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J. R. HIND, Esq., President, in the Chair.

John Alfred Barringer, Esq., Ackworth School, near Pontefract, Yorkshire;

Thomas Percy Gray, Esq., The Lodge, Clapham Road, Bedford; and

Robert Wigglesworth, Esq., St. Michael's House, Minster Yard, York,

were balloted for and duly elected Fellows of the Society.

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*On the Inclination of the Zodiacal Light.* By T.W. Backhouse, Esq.

I do not know whether the inclination of the Zodiacal Light to the ecliptic has been calculated before, but even if it has, an independent investigation will not be without its value.

With this view, I have examined my observations of the position of the central line or axis of the Zodiacal Light from 1867 to the summer of 1877. There are altogether 418 observations of various points of the central line (counting those most roughly observed as but half an observation): 360½ give northern latitude, average + 2°·32; 33½ give the latitude as 0°; and 24 give southern latitude, average −0°·94. The average of the whole is + 1°·95; and the average deviation from the ecliptic is 2°·06.

I have arranged the observations according to their distance from the Sun, and according to the Sun's longitude. A summary of them is given in the accompanying table, which shows, in degrees, the average latitude of the axis for every 30° of the Sun's longitude, and every 30° of distance following the Sun. The number of observations is given below to the left of each average.

TABLE I.

*Longitude of Zodiacal Light following the Sun.*

| Sun's Long. | 330° 345–316 | 300° 315–286 | 270° 285–256 | 240° 255–226 | 210° 225–196 | 180° 195–166 | 150 165–136 | 120° 135–106 | 90° 105–76 | 60 75–46 | 30° 45–16 | Aver. and Total. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 146–175 | | | +2·0 | −0·0 | −1·2 | −1·0 | | | | | | |
| | | | 1 | 4 | 4 | 1 | | | | | | |
| 176–205 | +2·2 | +1·3 | +0·3 | | +1·8 | +0·4 | +0·4 | | | | | |
| | 2 | 11 | 3 | | 22 | 27 | 10 | | | | | |
| 206–235 | +3·2 | +2·7 | +1·9 | | +3·0 | +2·0 | −0·1 | +0·0 | | | | |
| | 6 | 9 | 4 | | 1 | 8½ | 7 | 1 | | | | |
| 236–265 | +3·0 | +4·0 | +3·1 | +1·7 | | +2·5 | +1·0 | +1·3 | +2·8 | +2·0 | | |
| | 1 | 7 | 4 | 4 | | 3 | 7½ | 9 | 8½ | 1 | | |
| 266–295 | | +3·7 | +5·5 | | +1·5 | | +1·7 | +2·2 | +1·9 | +2·7 | | |
| | | 2 | 2 | | 1 | | 2 | 8½ | 12½ | 2½ | | |
| 296–325 | | | | | +1·3 | +1·6 | +1·5 | +2·8 | +2·2 | +1·9 | +2·4 | |
| | | | | | 3 | 5 | 1 | 5 | 23½ | 19½ | 3½ | |
| 326–355 | | | | +3·0 | +2·9 | +1·9 | +2·1 | +1·7 | +2·6 | +2·4 | +2·0 | |
| | | | | 1 | 5½ | 20½ | 11½ | 3 | 13½ | 35½ | 7½ | |
| 356–25 | | | | | +3·0 | +2·2 | +2·6 | +1·4 | +1·5 | +2·5 | +2·6 | |
| | | | | | 1 | 11½ | 10½ | 7 | 1 | 10½ | 5½ | |
| 26–55 | | | | | | +3·0 | +3·6 | +2·8 | +2·4 | | | |
| | | | | | | 2 | 5 | 4 | 4½ | | | |
| Average | +2·9 | +2·5 | +2·4 | +1·1 | +1·7 | +1·4 | +1·6 | +1·9 | +2·3 | +2·3 | +2·3 | +1·59 |
| Total | 9 | 29 | 14 | 9 | 37½ | 78½ | 54½ | 37½ | 63½ | 69 | 16½ | 418 |

On the theory that the Zodiacal Light is a solar appendage, of lenticular form, decreasing in brightness from the Sun; that its central plane maintains a constant position; and that the counter-glow is not a physical feature, but an apparent phenomenon, causing an increase of brightness at the point opposite the Sun—this table should present greater regularity than it does: only that, as the Zodiacal Light is so very indefinite, and observations of its central line therefore so inexact, we should expect the errors of observation to be proportionately very great. On this account, it is necessarily difficult to arrive at a correct idea of the inclination. First, I endeavoured to find the longitude of the ascending node, in the following different ways:—

A general examination of the table here given, ignoring those cases where the number of observations is small, seems to indicate that its most probable position is at long. 30°.

If the assumptions given above are correct, twice in the year the Earth must be in the axis of the Zodiacal Light; and at these times the latitude will be 0° at and opposite the Sun, and will reach its maximum, *plus* and *minus*, 90° off it. Careful examination of several tables shows that this is the case at no time of the year; the average position of the Sun agreeing best with this is long. 232°, giving the ascending node at long. 52°.

Using only those few observations that seemed likely to be the best on account of their great altitude, and confining my attention to those few of these that were about 90° off the Sun, on account of their possessing a fair brightness, the most probable longitude of the ascending node comes out at 55°.

On the hypothesis of the counter-glow with which I began, the central line there will be nearer the ecliptic than the true position of greatest density of the Zodiacal Light; how much nearer, will depend on the relative rates of concentration of the Zodiacal Light itself, and of the law determining the counter-glow. From the best observations of the counter-glow, the ascending node is at 37°.

The average of all these results is 43°.

But probably the best mode of examining the observations is to see whether the latitude of each point in the central line rises and falls among the stars in the course of the year, as it should do if the theories with which I started are correct. By this means any false results which might arise from constant errors, such as might be caused by the brightness of neighbouring stars or the presence of unsuspected branches of the Milky Way, will be got rid of. For this purpose, a table has been prepared, showing the way in which the central line should rise and fall according to theory; and I have compared the observed latitude of each point of the central line with this theoretical table, to see with what longitude, with reference to the ascending node, it would agree best. The results are shown in Table II., the first column of which gives the average longitude of the portion of the Zodiacal Light examined, and the second column the longitude, with refer-

ence to the ascending node, with which its variations agree best. The third column gives the longitude of the ascending node resulting from this.

TABLE II.

| Long. of Zodiacal Light: Average Position Observed. | Agrees best with Ω + | Ascending Node. |
|---|---|---|
| ° | ° | ° |
| 340 | 312 | 28 |
| 350 | About 35 | About −45 |
| 355 | −15 | 10 |
| 11 | −14 | 25 |
| 16 | About 0 | About 16 |
| 25 | 345 | 40 |
| 35 | 25 to 75 | 10 to −40 |
| 44 | 22, rather badly. | 22 |
| 50 | 70, variations slight. | −20 |
| 62 | 30 | 32 |
| 111 | 67 | 44 |
| 126 | About 100, variations slight. | About 26 |
| 147 | 100 | 47 |
| 164 | 90, very badly. | 74 |
| 178 | 135, rather badly. | 43 |
| 188 | 145 | 43 |
| 200 | 150, rather badly. | 50 |
| 215 | About 180, badly. | About 35 |
| | | Average 27 |

The best average of this table is 27°, which, combined with the average of the previous modes of examination, gives

$$\text{Long. ascending node} = 35^\circ.$$

These results seem sufficiently consistent to give considerable probability, though I think not amounting to proof, that the hypotheses on which we started are in the main correct, and that the ascending node of the Zodiacal Light is somewhere about long. 35°.

The inclination is more difficult to determine. Theoretically, it would be very simply determined from the observations of the Zodiacal Light at the time when the Sun's longitude is 35° or 215°; but these are so irregular that it is impossible to ascertain it exactly. These observations are best satisfied by

$$\text{Inclination} = 1^\circ.7;$$

but this requires the hypothesis of a constant error in all the observations, amounting to + 1°·3. Without any such hypothesis, the inclination comes out 2°·9, but in this case the probable error of the observations is much larger. It is obvious that either result is entitled to very little weight, the probable error in any case being very large.

If the Zodiacal Light is a solar appendage, we should not be surprised at finding its axis in the plane of the solar equator; but it is quite evident that such is not the case. We should have no reason for expecting it to be in the plane of the ecliptic rather than in that of any other planet's orbit; it would be more likely to be in the plane of the average position of the orbits of all the planets. But I have not calculated what this plane is, so as to avoid every bias in my investigations as far as possible.

Except on two nights, all the observations were made at Sunderland, lat. 55° N.

I have been aided in the calculations by J. Taylor, E. Dodshon, A. Gröne, and A. J. Edmunds.

*Sunderland:*
1881, *April* 27.

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## *Note on the Flexure of the Greenwich Transit Circle.*

By W. H. M. Christie, Esq.

Prof. Newcomb has recently called my attention to the practical difficulties attending the determination of the astronomical flexure by means of opposing collimators, and to the effect which the increase in the temperature of the air from the bottom to the top of the observing room might have on this observation. And he points out that if no change had been made in the adopted flexure at the time of the piercing of the cube of the Transit Circle, there would have been no striking change in the R−D correction. In his work on the "Declinations of Fixed Stars," pp. 35 and 36, Mr. Lewis Boss has examined the flexure and R−D corrections 1851–1872 with reference to Prof. Newcomb's suggestion, and has found that if the uniform flexure +0″·21 had been employed throughout the series, we should have had sufficiently good agreement between direct and reflection observations. This view entirely agrees with the conclusion at which I arrived in my paper on the Systematic Errors of the Greenwich N.P.D.'s from the discussion of the observations of circumpolar stars and of the Sun, before I was aware of Prof. Newcomb's suggestion or of Mr. Boss's work.

As further data are now available, I have thought it might be not without interest to compare the flexure and R−D corrections for the period 1851 to 1880. The following table is derived from that which I gave in the Addendum to my paper

(*Memoirs*, vol. xlv. p. 182) with the addition of the R—D correction for 1880—viz.

$$+0''.07 + 0''.54 \sin z \cos^2 z,$$

which has been recently computed.* I have converted the corrections from the law $b' \sin z \cos^2 z$ to $b \sin z$ by multiplying $b'$ by ·843. The R—D correction is thus the correction apparently required to the Adopted Flexure.

| | R—D $b \sin z$. | Adopted Flexure. | Apparent Flexure. | Weight. |
|---|---|---|---|---|
| | $''$ | $''$ | $''$ | |
| 1865 | −0·65 | +0·76 | +0·11 | 1 |
| 1861–64 | −0·45 | +0·56 | +0·11 | 4 |
| 1857–60 | −0·39 | +0·56 | +0·17 | 4 |
| 1851–56 | −0·29 | +0·58 | +0·29 | 6 |
| 1879–80 | +0·39 | 0·00 | +0·39 | 2 |
| 1871–78 | +0·43 | −0·12 | +0·31 | 8 |
| 1866–70 | +0·65 | −0·37 | +0·28 | 5 |

Under the head "Apparent Flexure" is given the flexure correction indicated by the reflection observations of stars, found by adding the coefficient of the R—D correction to the Adopted Flexure.

Taking the means of the Apparent Flexure for the whole period and for the periods before and after the piercing of the cube, we have—

| | | | | |
|---|---|---|---|---|
| Mean Flexure | 1851–1880 | ... | ... | $+0''.25$ |
| „ | 1851–1865 | ... | ... | +0·20 |
| „ | 1866–1880 | ... | ... | +0·31 |

From this it would seem that if any change of flexure occurred on the piercing of the central cube of the Transit Circle, it was in the opposite direction to that indicated by the Collimator determinations, and was simply a small increase in the coefficient. With regard to the apparent change of sign indicated by the collimators, it is to be remembered that before the piercing of the cube the observations were made with collimators of four inches' aperture and with the instrument raised, and that after the piercing they were made with collimators of seven inches' aperture and through the openings in the cube. Now it is found that in the determination of collimation-error there is a discordance between the readings taken with the instrument raised and through the cube respectively, and this difference affects the result for collimation-error in the mean by $0''.30$. If abnormal refraction resulting from inequalities of temperature has anything

* The Adopted Flexure for 1851 and 1852 was really $0''.73$, instead of $0''.50$. I have here corrected for this error.

to do with this discordance, it seems not unnatural to suppose that it might produce even a larger effect in zenith distance. No such discordance, however, has been detected, though some observations with the instrument raised, and through the cube, have been made for this purpose.

The recent determinations of flexure by the collimators do not, however, differ much from that indicated by the R−D correction. The following are the results since the beginning of 1879:—

| | | | |
|---|---|---|---|
| 1879, April 22 | +0″·26 | 1880, July 22 | +0″·04 |
| 23 | +0·07 | Dec. 6 | +0·20 |
| 1880, April 6 | +0·19 | 1881, May 2 | +0·13 |
| May 6 | +0·04 | May 10 | +0·18 |

The mean of these is +0″·14, whilst the mean from the R−D correction is +0″·25.

With reference to the question of the distribution of temperature in the room, the following readings of thermometers mounted respectively near the north and south collimators and the central cube, as well as of the exterior thermometer, were taken on 1881 May 4 and on May 10 at the beginning and end of the observations:—

| | N. Coll. | Cube. | S. Coll. | Exterior. |
|---|---|---|---|---|
| h | ° | ° | ° | ° |
| May 3 22 | 49·6 | 49·8 | 49·8 | 50·8 |
| 9 22 | 47·8 | 48·3 | 47·5 | 47·9 |
| 9 23 | 48·6 | 48·9 | 48·6 | 49·1 |

The readings of the thermometers were inadvertently omitted on May 2; those on May 4 are given as illustrating the distribution of temperature under nearly similar conditions. Considering the difficulty of determining the true temperature of the air in the neighbourhood of large masses of iron and stone, the readings of the interior thermometers seem to correspond as closely as can fairly be expected.

In conclusion, I must point out that although the R−D discordance may reasonably be explained as the effect of error in the flexure determination, there yet remains to be accounted for the error of nearly 1″ at Z.D. 60° North, which is indicated by the circumpolar observations 1868–1876, and this is to my mind a much more serious matter.

*Blackheath:*
1881, *May.*

*Further Remarks on Mr. Stone's Alterations of Bessel's Refractions.*
By W. H. M. Christie, Esq.

Mr. Stone, in his Reply to my paper "On Mr. Stone's Alterations of Bessel's Refractions," published in the March No. of the *Monthly Notices*, vol. xli. pp. 272–281, has tacitly conceded so many of the points which I have urged that I should hardly have considered it necessary to say anything further on the subject if it had not been that he asks for explanations of some of my statements.

With reference to paragraphs 1 and 2 in the remarks on p. 95 of my paper, I can only express my regret that they have been misunderstood. They were not intended to cast any reflection on Mr. Stone, but to call pointed attention to the omission of a number of circumpolar observations from the collected results in the Latitude Investigation given in the Greenwich "Introduction." With the exception of the years 1857, 1858, and 1859, the invariable practice has been, I believe, to collect all the results of circumpolar observations in the Latitude Investigation. As I am not the only person who has felt a difficulty from the absence of all explanation in Mr. Stone's original paper on Bessel's Mean Refractions of the source from which he derived his results, and as others might be misled by the omission of the observations from the Greenwich "Introductions," I left the statement of my difficulties as it originally occurred to me, and gave the explanation afterwards. I should be sorry to think that my statement would bear such a construction as Mr. Stone seems to put on it. It was meant to be explanatory, not critical, for it is obvious that serious criticism would be wasted when Mr. Stone himself is reduced to saying "I have felt throughout that I must have applied all the corrections which could be applied with certainty to the results." Even now Mr. Stone does not say whether he used the old law for R—D, or the new, or a mixture of the two. Mr. Stone had shown to his own satisfaction that his results were right and mine were wrong, and under these circumstances I desired to explain that there was nothing incompatible in two sets of results obtained with different data and different corrections. I do not attach the same importance to these particular observations that Mr. Stone does, and I regard them as simply a small portion of the chain of evidence on the R—D correction and Bessel's Refractions.

The next point that calls for explanation has reference to Mr. Stone's remarks on pp. 275, 276, in which, after imputing to me a statement which I have not made, and which, in fact, conveys a totally different meaning from my actual words, he proceeds to call attention to a serious fallacy which invalidates my work upon these questions of refractions. The fallacy in question is to be found in the words which Mr. Stone puts into my mouth, but not in what I have actually said. Mr. Stone

argues that *Bessel's Refractions unaltered* do not represent the Greenwich observations, because, as Mr. Main has shown, alterations are required beyond Z.D. 82°. This is perfectly true, and, as Mr. Stone well knows, I have over and over again stated that I adopt Mr. Main's corrections at very low altitudes, considering that they show Bessel's *law* of refraction to be erroneous. This, however, does not in the least affect the question of the accuracy of Bessel's *constant* of refraction, or, as it is sometimes expressed, Bessel's *mean* refractions. Mr. Stone's argument, therefore, falls to the ground unless he can show that I have maintained that *Bessel's Refractions unaltered* represent the Greenwich observations. He quotes me thus, p. 275 (the italics are mine):—

"Mr. Christie considers my statement 'that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of *Bessel's Refractions unaltered*,' as 'really quite misleading.'"

What I really said was this, under the heading "First, as regards the value of the *constant* of refraction," on p. 97:—"In the first place, he [Mr. Stone] brings forward his discussion of the observations 1857–1865, and appeals to his residuals and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the *mean refractions*;" and further on in the same paragraph I add:—"So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations." The reference here is to Mr. Stone's statement in his paper "On *Mean* Refractions," April 1880, *Monthly Notices*, vol. xl. p. 336:—"The recent Cape Observations do appear to offer grounds for serious doubts about the necessity of decreasing Bessel's Refractions, *for small zenith distances*, by 0·0053; but the evidence thus collected is not certainly such as should outweigh the accumulated evidence of the Greenwich circumpolar observations."

There is another statement made by Mr. Stone which does not seem to be characterised by the accuracy which is desirable in such matters. On p. 276 he makes this statement: "With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0″·9 to over 2″." This statement is incorrect as regards the facts, and is calculated to convey the erroneous impression that I had not applied corrections to reduce the observations to the same set of refractions throughout. In the first place, the actual corrections range from 0″·9 to 1″·12, *not* "from 0″·9 to over 2″," and the appropriate correction has been applied to each individual result, so that there is no error from this cause; and, in the second place, the observations 1862–1865 have *all* been reduced with the same refractions—viz. Bessel's with Main's corrections below Z.D. 82°. It is curious that Mr. Stone should himself have done the very thing for which he reproaches

me, for he has grouped together observations above 82° Z.D. and observations below 82° Z.D. in his last group N.P.D. 43° to 46° 29′, and, what is a more serious oversight, he has grouped together observations reduced with two different laws for R−D discordance, without, as far as I can make out, applying any correction for the difference, which amounts to 0″·36. With reference to my selection of the groups of years 1851–1861 and 1862–1865, I may explain that one of the principal objects of my paper on the Systematic Errors of the Greenwich N.P.D.'s was to examine the validity of the R−D correction, and that the law was changed at the beginning of 1862. I did not propose to myself any discussion of refractions at low altitudes, and the examination of the results which Mr. Stone obtained in 1867 from the observations 1857–1865 was quite an incidental matter. Had I intended to discuss the refractions below 80° Z.D., I should have adopted Mr. Main's exhaustive treatment rather than the method followed by Mr. Stone, and I should have included in the discussion the Greenwich circumpolar observations below Z.D. 85°, of which in the period 1857–1865, there were 33 above the Pole and 21 below. It would be interesting to know why Mr. Stone has omitted these observations, which have an important bearing on the special question which he discusses.

But though I do not profess to discuss the refractions at very large Z.D., for which the data accumulated since Mr. Main's Memoir seem hardly sufficient, I must take exception to Mr. Stone's assumption that observations extending to Z.D. 78½° are not sufficient for discriminating between Mr. Stone's *constant* of refraction and Bessel's. As a matter of fact, Mr. Stone's alteration would affect the residual at Z.D. 78½° by 1″·0. But my reason for not discussing the observations below Z.D. 78½° in the period 1840–1847 (I have gone as far as Z.D. 84° in the periods 1851–1861 and 1862–1865) is that I did not see that I could add anything to Mr. Main's exhaustive discussion of all the observations below Z.D. 70° in the period 1836–1854. As Mr. Stone studiously ignores my reference to this important investigation as supplementing my own work, I will here give the residuals obtained by grouping Mr. Main's results for individual stars, in connection with my residuals for the periods 1840–1847 and 1851–1861:

*N.P.D. above Pole—N.P.D. below Pole.*

*With Bessel's Refractions and Main's Corrections below Z.D. 82°*

| Mean N.P.D. | 2° | 7½° | 12° | 21° | 28° | 31° | 33° | 37° | 41° | 44½° |
|---|---|---|---|---|---|---|---|---|---|---|
| $\alpha$ | +″·15 | +″·05 | −″·07 | −″·18 | −″·07 | +″·02 | +″·37 | +″·20 | ″ | ″ |
| $\beta$ | | | | | | | +″·13 ±″·13 | +″·22 ±″·13 | −″·22 ±″·16 | −″·09 ±″·16 |
| $\gamma$ | −″·10 | +″·02 | +″·04 | −″·27 | | +″·15 | | +″·04 | +″·02 | −″·02 |

The residuals $\alpha$ and $\gamma$ are for the periods 1840–1847 and

1851–1861 ; the residuals $\beta$ are formed from Mr. Main's results, *Memoirs R.A.S.* vol. xxvi., Table viii. pp. 122–23, giving weights by the formula $W = \frac{1}{2\epsilon^2 + \frac{e^2}{n}}$, where $e$ is the probable casual error of an observation S.P., and $\epsilon$ (which is taken $= 0''{\cdot}22$) is the probable error of the result above pole and the probable systematic error of an observation S.P. The probable error of the residual inferred from the weight of the group is appended with the sign $\pm$. Each of the groups here treated corresponds to two of Mr. Main's groups in his Table x. (cont.), p. 129.

I submit that these residuals afford stronger evidence in favour of Bessel's constant of refraction than those from the observations 1857–1865 do in favour of Mr. Stone's diminished constant. And I cannot help thinking that he should have weighed the evidence of these 19 years, from 1836–1854, a little more carefully before he proposed a change which has turned out so unfortunately. It is to be noted, too, that his residuals B are seriously affected by the assumed law for the R − D correction, which I have given reasons for considering to be erroneous.

As regards his residuals C, 1868–1876, I must ask why he omits to give the weights of the several groups. I should imagine that no practical astronomer would accept Mr. Stone's residuals without inquiring what are the weights of the respective groups, what corrections have been applied, and what residuals would be given by an alternative hypothesis. The importance of this last test I have illustrated by reference to Mr. Stone's discussion of the Melbourne circumpolar observations* as well as of those now in question. In both cases I have shown that Bessel's Refractions with Main's corrections below Z.D.82° give smaller residuals.

There is only one point which calls for further remark, and that is with reference to the adopted co-latitude, for the years 1868–1876. Here Mr. Stone has apparently misapprehended my remarks and has so paraphrased my words as to convey a very different meaning. What I said was, not that the apparent change in the co-latitude "is due *solely* to a better distribution of the weights," but that it "is simply due to a change in the system of weights *and* to the increased number of observations of wide circumpolars," and I add that it began "in 1872, when special attention was given to the observation of *wide* circumpolars." No doubt it seems very specious to use the co-latitude given by the observations, but if this effective co-latitude does not agree with the real co-latitude of the Greenwich Observatory, it seems to me to show that there is a systematic error in the observations, which we ought rather to try to detect than to

* There is a small printer's omission in my paper, *Monthly Notices*, vol. xli. p. 98, line 31 : "Each has a probable error corresponding to weight,' should read, "Each has a probable error corresponding to weight 1, the figure 1 having dropped out in printing.

mask by the method which Mr. Stone adopts. And I would point out that Mr. Stone does not use what he terms the effective co-latitude—*i.e.* the difference between the circle-readings for the zenith point and the polar point—for that can only be given by observations of stars close to the polar point and not by stars 20° from this point. As I have pointed out, the close circumpolar stars show no change in the co-latitude, and thus there is no change in the difference of circle-readings for the zenith point and the polar point of the circle. But there is a change of nearly 1″ (as the curve given in my paper "On the Systematic Errors of the Greenwich N.P.D.'s" shows) in the difference of circle-readings for the zenith point and a point 22° below the pole at Z.D. 68° north. And this has nothing to do with the adopted zenith points, since the same change is shown in the distance between the polar point and the point 22° below the pole. Mr. Stone's argument that if the method of correcting the nadir point was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, amounts to this, that if one thing is right other things cannot be wrong. I am glad, however, to find that Mr. Stone does not now dispute the validity of the method of correcting the nadir-point observation.

I now come to the statement which I gave respecting the errors of the micrometer screws, and which Mr. Stone thinks is not made with the precision which is desirable.

The statement in question—"In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at $0^r$ of the microscope-micrometers"—means that the following notice was put up in the Transit Circle room: "The Nadir Point observation is to be taken at 179° 40′ + $0^r$." As Mr. Stone is, I believe, responsible for this notice, no doubt he can give its precise meaning. In my paper "On the Effect of Wear on the Micrometer Screws of the Greenwich Transit Circle" (*Monthly Notices*, vol. xxxvii. p. 20) it was sufficient for me to show that there was a preponderance of a large number of observations between $0^r$ and $1^r$ of Microscope A as it read in 1875, or between $0^r{\cdot}2$ and $1^r{\cdot}2$ of Microscope A as it read during a considerable part of 1868. An examination of the curves I have given will show that when the corrections indicated in the text (p. 20) are applied to the 1868 curve (viz. −0″·4 approximately at $0^r$, and +0″·2 at $5^r$, with intermediate corrections between these two points) the 1875 curve begins to rise above the 1868 curve (indicating increased wear of the screws) at about $0^r{\cdot}4$ of Microscope A as it read in 1868, and does not meet this curve again till about $1^r{\cdot}2$. It must be understood, however, that no great precision is to be expected in such a case, on account of accidental errors in the curve, and I have therefore contented myself with specifying the part between $0^r$ and $1^r$ as indicating extra wear since 1868. I have nowhere said anything about readings of Microscope A less than $0^r{\cdot}2$, and therefore Mr.

Stone's interesting collection of such readings does not in the least concern me. Let us now take the nadir observations 1868, Feb. 3, to 1869, Dec. 31, with reference to my actual statement, and not with reference to Mr. Stone's interpretation of it. In the period cited there were 559 determinations in all: 543 of these were made at readings (of Microscope A) $1^{r}\cdot2$ and under, 520 at readings $1^{r}\cdot0$ and under, 488 at readings $0^{r}\cdot9$ and under, 448 at readings $0^{r}\cdot8$ and under, 334 at readings $0^{r}\cdot6$ and under, and 283 at readings $0^{r}\cdot4$ and under. I think it is evident from this that there was a large preponderance of observations in the part of the screws I have specified, more especially since the nadir-point observation was usually taken twice a day in the latter part of the period 1868–1875. It seems a pity that Mr. Stone did not make himself better acquainted with the facts before making the confident assertion that "whatever may have been the state of the screws near $0^{r}\cdot0$ in 1876, it certainly was not caused by the 4,000 observations made near $0^{r}\cdot0$ at my suggestion."

With regard to the practice introduced in 1868 of taking the nadir point at $0^{r}$, Mr. Stone states that it was done to avoid very large corrections for run. I had heard this reason given, but I should have been loath to attribute it to Mr. Stone, for it implies the serious fallacy that the real reference point for runs is $0^{r}$ instead of being about $2^{r}\cdot5$, the middle of the range over which the observations are distributed. It is perfectly true that for mere convenience of computation the correction for runs is reckoned from $0^{r}$ so as to avoid negative signs, but the real correction for runs, as affecting any particular observation, is the difference between the correction reckoned from $0^{r}$ and the average correction for observations of stars. I had stated this in the very passage which Mr. Stone quotes, and had given it as the reason for my suggesting that the nadir observation should be taken about $2^{r}\cdot5$, but as Mr. Stone takes no notice of it, I am obliged again to call attention to the point.

After this statement of Mr. Stone's I am not so surprised as I should otherwise have been at the difficulty he experiences in understanding how the corrections for runs enter into the corrections for errors of micrometer screws, though I thought I had sufficiently explained the matter in my paper of 1876. Mr. Stone is quite right in supposing that the runs had not been corrected for errors of screws before their application, but he is quite wrong in supposing that there was any oversight in the matter, and by his implying that the runs ought to have been so corrected for errors of screws he seems to me to show that he does not appreciate the nature of the problem.

The problem really was this: To determine the corrections applicable to the Greenwich N.P.D.'s on account of errors of the micrometer screws, these N.P.D.'s being affected by error of the screws at the point at which the observation was made, and by the error in the runs resulting from error of the screws at $0^{r}$ and

4r·9. For this purpose simultaneous observations were made with the ordinary micrometers, and with the four supplementary micrometers, the latter being presumably free from error. The observations with the ordinary micrometers were reduced with the runs derived from these worn screws as in the ordinary course of work, and the observations with the supplementary micrometers were reduced with the runs from their own screws. By the comparison of the two sets the curve was laid down, "the ordinates of which give the corrections to the circle-reading corresponding to the revolutions and tenths of Microscope A" (*Monthly Notices*, vol. xxxvii. p. 19).

There is no oversight in this. The method was adopted advisedly with the object of determining, not the actual errors of the micrometer screws, but the corrections to the circle-readings on account of these errors. The determination of the actual errors has no practical importance, and could not have been accomplished without special apparatus and a serious interruption to the regular observations. And even if the actual errors had been determined it would have been necessary to correct the runs as well as the micrometer-readings for every observation, and these two corrections would together be equal to the single correction actually applied.

There may, however, be a small effect from error in the runs, if the runs be taken at different parts of the screws. I have expressly referred to this in my paper of 1876 as probably explaining a small difference between the errors at 0r and 5r in 1868 and 1875 respectively. Since the correction for runs is adopted from a considerable number of determinations extending usually over a fortnight, it may safely be assumed that the adopted run will apply sensibly to the same part of the screws.

As regards the other remarks which Mr. Stone makes on the alterations of Bessel's Refractions, I do not find that he has brought forward any new points which call for further notice after the somewhat lengthy discussions of Bessel's constant and law of refraction contained in my two previous papers on the subject.

*Blackheath:*
1881, *May.*

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*On the Supposed Difference in the Refractions North and South of the Zenith of Melbourne.* By A. M. W. Downing, Esq.

In the *Monthly Notices* for December 1867 there is a paper by Mr. Stone "On Bessel's Mean Refractions," containing (amongst other things) a discussion of the North Polar Distances of stars observed at Melbourne during the years 1863, 1864, and 1865. As the result of this discussion it appeared that from the North Polar Distances of stars observed above and below the pole, the refractions of the *Tabulæ Regiomontanæ* should be diminished by 0·00372 × Refraction; whilst from the comparison

of Greenwich and Melbourne results it appeared that, to the north of the zenith the tabular refractions should be diminished by 0·00914 × Refraction, to satisfy the Melbourne observations. The correction to the adopted co-latitude, corresponding to the above diminution of the tabular refractions derived from the observations of circumpolar stars, was +0″·149. These results were actually used in the reduction of the observations of stars whose places are given in the First Melbourne General Catalogue for the epoch 1870, it being assumed that the mean refractions were really different north and south of the zenith. A discussion of the same Melbourne observations by Prof. Gyldén (*V. J. S.* Bd. iv. Heft 2) was considered to be confirmatory of Mr. Stone's results, and also showed that* "a similar discrepancy was apparent in the Cape of Good Hope and the St. Helena observations, thus indicating the cause to be a general one." With regard to the Cape of Good Hope observations, it must be remarked that Prof. Gyldén's investigations only refer to the observations made with the old Mural Circle, and that Mr. Stone has shown (*Monthly Notices*, Dec. 1879) that, as far as the observations made under his direction are concerned, there is no sensible difference between the mean refractions north and south of the zenith, the observations on both sides being very fairly represented by Bessel's Refractions unaltered. And I have no doubt that if a first-class Transit Circle were erected on the site of Johnson's Observatory at St. Helena, we should have very similar results.

My attention was drawn to the large correction which has been applied to the refractions of the *Tabulæ* in the formation of the N.P.D.'s of the Melbourne Catalogue for stars north of the zenith by finding, from a discussion of the star places of the Catalogue (*Monthly Notices*, Jan. 1879), that the N.P.D.'s of stars passing the meridian near the north horizon appeared to be systematically too large, and therefore that the adopted refractions were too small. The same thing appears from a comparison of the Melbourne and Washington observations made by Mr. Lewis Boss ("Declinations of Fixed Stars," p. 67), from which he finds that the refractions of the *Tabulæ* must be diminished by 0·00733 × Refraction to satisfy the Melbourne observations. (With regard to the assumption of a difference in the refractions north and south of the zenith, Mr. Boss aptly remarks that, "though possible on *a priori* grounds, this hypothesis is open to serious objections, when we consider the difficulty of determining the correction to the adopted refractions independently at the latitude of Melbourne.") There cannot be much doubt, therefore, but that the correction applied to the stars on the north side, derived from the observations made 1863–1865, is too large to satisfy the observations made during the whole period 1863–1870, the results of which are embodied in the Melbourne Catalogue. Under these circumstances it becomes a matter of interest and importance to investigate the subject in as complete a manner as

* Introduction, *Melbourne Cat.* p. iv.

the available materials render possible; and I have therefore not hesitated to avail myself of the opportunity for entering on the discussion presented by the publication of another volume of Melbourne Astronomical Results.

This volume (the fifth of the series) contains the results of observations made during the years 1871–1875, both inclusive. The instrument and methods of observing remain substantially unaltered, and therefore need no detailed description here. It may, however, be remarked that special attention has been paid to the observation of wide circumpolars, and of stars passing the meridian near the north horizon, for the purpose of determining the refraction both to the north and to the south of the zenith. There is unfortunately no provision for the observations of stars by reflexion. This is a most serious omission, and must greatly diminish our confidence in the Melbourne results. Reflexion observations are most essential, both for the accurate determination of zenith points (in combination with the nadir observation), and also for detection of systematic errors in the circle-readings, whether these arise from uncorrected flexure or other causes. I hope that before long we shall have reflexion observations made regularly and uninterruptedly at Melbourne.

With regard to the horizontal flexure, it is stated in the Introduction to this volume that its amount was determined, by the method of opposing collimators, once a week from the beginning of 1871 to the end of 1874, and once a month during 1875; a mean value was used for the interval between the observations, as in the case of the runs. It is unfortunate that no table of these observations, or of the results derived from them, is given, so that we have no information as to the stability or otherwise of this important correction. Those who are concerned with the printing of astronomical observations would do well to remember that it is very desirable that all matters relating to the adjustment or stability of instruments should be given in considerable detail.

I now proceed to the discussion of the observations of circumpolar stars contained in this volume. The places of the stars as found from each year's observations have been taken from the Annual Catalogues, and reduced to a common epoch 1873·0, the results for upper and lower transit being of course kept separate; mean places have then been computed for each star for 1873·0, and the difference N.P.D. at upper transit *minus* N.P.D. at lower transit found. The stars have next been arranged in order of N.P.D., and divided into groups, each group embracing about 5° of N.P.D. The results of each group have been combined, so that corresponding to a definite N.P.D. we have a definite discordance, and for the purposes of the present investigation it is assumed that these discordances arise from error in the adopted co-latitude, and error in the adopted refraction. The co-latitude adopted in this volume is that originally found for the Melbourne Observatory—viz. 52° 10′ 6″·6—and the refractions used are those of Bessel's *Tabulæ Regiomontanæ* unaltered. The weights

assigned to the places of individual stars are found from the expression for the probable error of a Melbourne result given by Prof. Gyldén in the paper referred to above (*V. J. S.* Bd. iv. 102), viz. :—

$$\epsilon^2 = (0\cdot26)^2 + \frac{1}{n}\left\{0\cdot263 + 0\cdot0467\left(\frac{\text{refr.}}{100}\right)^2\right\},$$

where $n$ is the number of observations on which the result depends. The weight of a discordance N.P.D. upper *minus* N.P.D. lower for a single star is

$$w = \frac{1}{\epsilon_U{}^2 + \epsilon_L{}^2},$$

and the weight to be assigned to a group

$$W = \Sigma w.$$

The following table gives the discordances and the corresponding N.P.D.'s found in the manner just described; and I have added for comparison the results obtained by applying the corrections found by MM. Stone and Gyldén from their discussion of the observations 1863–1865.

Mr. Stone's corrections are given in a tabular form in the Introduction to the Melbourne Catalogue, p. v. Prof. Gyldén's definitive corrections are—

$$\delta c = -0''\cdot16$$
$$\kappa = -0\cdot00314$$
$$\delta\beta = -0''\cdot783,$$

where $\delta c$ is the correction to the adopted co-latitude, $(1+\kappa)R$ is the true refraction (R being the adopted value), and $\delta\beta$ the correction to the adopted horizontal flexure. A table of these corrections is given by Prof. Gyldén (*V. J. S.* Bd. xi. 184) from which the requisite quantities have been taken.

| | N.P.D. | No. of Stars. | Upper − Lower. I. | II. | III. | Zenith Distance. Upper. | Lower. |
|---|---|---|---|---|---|---|---|
| | ° ′ | | ″ | ″ | ″ | ° ′ | ° ′ |
| 1 | 177 6 | 32 | +0·44 | −0·42 | −0·82 | 49 16 | 55 4 |
| 2 | 173 7 | 35 | +0 68 | −0·20 | −0·59 | 45 17 | 59 3 |
| 3 | 168 20 | 10 | +0·14 | −0·78 | −1·13 | 40 30 | 63 50 |
| 4 | 162 39 | 7 | −0·18 | −1·21 | −1·54 | 34 49 | 69 31 |
| 5 | 157 12 | 4 | +0·61 | −0·61 | −0 93 | 29 22 | 75 58 |
| 6 | 152 38 | 5 | +0·89 | −0·65 | −0 93 | 24 48 | 79 32 |
| 7 | 148 54 | 5 | +1·43 | −0·61 | | 21 4 | 83 16 |
| 8 | 146 4½ | 3 | +3·67 | +0·70 | | 18 14 | 86 5¾ |

I. Results as printed.
II. „ with Stone's corrections.
III. „ with Gyldén's corrections.

If we now assume that

$$\frac{x}{2} = \text{correction to adopted co-latitude},$$
$$R = \text{tabular refraction at upper transit},$$
$$R\,(1-y) = \text{true refraction at upper transit},$$
$$R_1 = \text{tabular refraction at lower transit},$$
$$R_1\,(1-y) = \text{true refraction at lower transit},$$

then our equations of condition are

$$x + y\,(R + R_1) = \text{N.P.D. upper} - \text{N.P.D. lower}.$$

In the last group (8) I now substitute for the mean refractions of Bessel's Supplementary Table those of the *Fundamenta* multiplied by the factor requisite to bring them up to the system of the *Tabulæ*, so that the refractions are then all found from the same system. The coefficients of $y$ are computed with barometer and thermometer readings at their mean values for Melbourne; it would be more satisfactory to use the actual readings for the last two groups, but the want of the requisite detailed information renders this impossible.

The following are then the equations of condition:—

| | Equations of Condition. | Weights. | Residuals. |
|---|---|---|---|
| 1 | $x + 148\,y = +0''.44$ | 149 | $-0''.05$ |
| 2 | $x + 153\,y = +0.68$ | 123 | $-0.27$ |
| 3 | $x + 165\,y = +0.14$ | 40 | $+0.33$ |
| 4 | $x + 192\,y = -0.18$ | 25 | $+0.78$ |
| 5 | $x + 242\,y = +0.61$ | 15 | $+0.23$ |
| 6 | $x + 326\,y = +0.89$ | 14 | $+0.34$ |
| 7 | $x + 473\,y = +1.43$ | 14 | $+0.50$ |
| 8 | $x + 724\,y = +4.38$ | 6 | $-1.26$ |

Proceeding to solve these by the method of least squares, I find for the normal equations

$$386\,x + 71431\,y = 218.21$$
$$71431\,x + 16797189\,y = 57332.24,$$

whence

$$x = -0''.311 \quad \pm 0''.203$$
$$y = +0.00474 \pm 0.00097.$$

If $y$ be supposed to be without error, the probable error of $x$ becomes $\pm 0''.092$. The sum of squares of residuals (the weights of course being taken into account) is 44·43.

By discussing all the available observations of circumpolar stars made during the years 1871–1875 in the above manner, we therefore find that the adopted co-latitude ought to be diminished by $0''.16$, and that Bessel's Refractions ought to be diminished by

0·00474 × Refraction. It will be remarked, however, that the residuals are not very satisfactory, and that the observations are affected by a considerable systematic error, not depending on refraction, in the neighbourhood of N.P.D. 165°.

For the purpose of comparison with the Greenwich Nine-Year Catalogue the Melbourne results, for stars which are contained in that catalogue, have been reduced to the epoch 1872·0; and mean places computed as in the case of circumpolar stars. The differences Greenwich *minus* Melbourne have then been found; the Greenwich places being corrected by the quantities given in the Addendum to the Introduction to the Nine-Year Catalogue, so that the refractions are Bessel's unaltered. Proceeding in the same manner as for the circumpolar stars, the results have been combined in groups, the weights for the Greenwich observations being found from the following expression for the probable error of a result

$$\epsilon^2 = (0\cdot26)^2 + \frac{e^2}{n},$$

where $n$ is the number of observations, and $e$ is taken from Mr. Stone's values of the probable errors of Greenwich observations at different zenith distances; whilst for the Melbourne observations the formula cited above (in the discussion of the circumpolar stars) has been used. Then for the combination we have

$$w = \frac{1}{\epsilon_G^2 + \epsilon_M^2}, \text{ and } W = \Sigma w.$$

The following table exhibits the results of this comparison :—

| | N.P.D. | No. of Stars. | Greenwich−Melbourne. I. | II. | III. | Zenith Distance. Greenwich. | Melbourne. |
|---|---|---|---|---|---|---|---|
| | ° ′ | | ″ | ″ | ″ | ° ′ | ° |
| 1 | 41 33 | 3 | +3·72 | −2·99 | +0·72 | 3 2 | 86 13½ |
| 2 | 44 42 | 7 | +2·61 | −1·33 | +0·23 | 6 11 | 83 8 |
| 3 | 47 27 | 8 | +2·47 | −0·37 | +0·41 | 8 56 | 80 23 |
| 4 | 50 55 | 7 | +1·22 | −0·85 | −0·53 | 12 24 | 76 55 |
| 5 | 57 20 | 6 | +0·87 | −0·47 | −0·56 | 18 49 | 70 30 |
| 6 | 62 14 | 20 | +1·08 | +0·06 | −0·19 | 23 43 | 65 36 |
| 7 | 67 31 | 13 | +1·37 | +0·59 | +0·21 | 29 0 | 60 19 |
| 8 | 72 20 | 9 | +1·27 | +0·65 | +0·21 | 33 49 | 55 30 |
| 9 | 76 59 | 16 | +0·90 | +0·40 | −0·08 | 38 28 | 50 51 |
| 10 | 82 7 | 16 | +0·56 | +0·16 | −0·34 | 43 36 | 45 53 |
| 11 | 87 24 | 16 | +0·48 | +0·18 | −0·34 | 48 53 | 40 26 |
| 12 | 92 47 | 9 | +0·01 | −0·21 | −0·73 | 54 16 | 35 3 |
| 13 | 98 25 | 11 | +0·60 | +0·45 | −0·06 | 59 54 | 29 25 |
| 14 | 102 51 | 4 | +0·52 | +0·42 | −0·06 | 64 20 | 24 59 |
| 15 | 107 9 | 11 | +0·40 | +0·35 | −0·10 | 68 38 | 20 41 |
| 16 | 112 50 | 6 | +0·06 | +0·07 | −0·36 | 74 19 | 15 0 |
| 17 | 117 1 | 8 | +0·04 | +0·09 | −0·30 | 78 30 | 10 49 |
| 18 | 120 18 | 1 | +0·96 | +1·07 | +0·67 | 81 47 | 7 32 |
| 19 | 124 9 | 1 | −2·10 | +1·99 | +2·31 | 85 38 | 3 41 |

I. Greenwich corrected, Melbourne as printed.

II. „ „ „ with Stone's corrections.

III. „ „ „ with Gyldén's corrections.

Groups (18) and (19) containing only one star in each—viz. *Fomalhaut* and *α Columbæ* respectively—have not been used in the subsequent computation.

Now, if we assume

True refraction for Greenwich = tabular $(1-x)$ = R $(1-x)$,

„ „ Melbourne = tabular $(1-y)$ = $R_1(1-y)$,

Correction for relative error of adopted latitudes = $\delta l$,

then the equations of condition are

$$Rx + R_1 y + \delta l = \text{Greenwich} - \text{Melbourne}.$$

The discordances are of course taken from column I. of the table, except that in the case of (1) the result of substituting the mean refractions of the *Fundamenta* brought up to the system of the *Tabulæ* for those of the Supplementary Table has been used; also the Greenwich results have been corrected for assumed error of the thermometer (+0°·55). In computing R and $R_1$ the barometer and thermometer readings are taken at their mean values for the two Observatories. The equations of condition then become:—

| | Equations of Condition. | Weights. | Residuals. |
|---|---|---|---|
| 1 | $3x + 723y + \delta l = +5''.22$ | 6 | −0″.50 |
| 2 | $6x + 444y + \delta l = +2.61$ | 26 | +0.42 |
| 3 | $9x + 325y + \delta l = +2.48$ | 31 | −0.22 |
| 4 | $13x + 241y + \delta l = +1.23$ | 36 | +0.50 |
| 5 | $20x + 160y + \delta l = +0.89$ | 33 | +0.32 |
| 6 | $25x + 126y + \delta l = +1.10$ | 92 | −0.10 |
| 7 | $32x + 101y + \delta l = +1.41$ | 66 | −0.56 |
| 8 | $39x + 83y + \delta l = +1.31$ | 53 | −0.57 |
| 9 | $46x + 70y + \delta l = +0.95$ | 88 | −0.28 |
| 10 | $55x + 59y + \delta l = +0.62$ | 94 | −0.01 |
| 11 | $66x + 49y + \delta l = +0.55$ | 81 | +0.01 |
| 12 | $80x + 40y + \delta l = +0.09$ | 48 | +0.42 |
| 13 | $100x + 32y + \delta l = +0.71$ | 56 | −0.22 |
| 14 | $120x + 27y + \delta l = +0.65$ | 16 | −0.16 |
| 15 | $147x + 22y + \delta l = +0.56$ | 48 | −0.07 |
| 16 | $203x + 15y + \delta l = +0.27$ | 26 | +0.25 |
| 17 | $277x + 11y + \delta l = +0.34$ | 31 | +0.25 |

Proceeding to solve these by the method of least squares, I find

$$6636741\,x + 2629857\,y + 54905\,\delta l = 34428{\cdot}67$$
$$2629857\,x + 18102512\,y + 84176\,\delta l = 135055{\cdot}82;$$

whence

$$x = +0.00237 - 0{\cdot}00682\,\delta l$$
$$y = +0{\cdot}00711 - 0{\cdot}00366\,\delta l.$$

If

$$\delta l = +0{\cdot}''156 \pm 0{\cdot}''102,$$

as found from the observations of circumpolar stars (assuming that the Greenwich latitude requires no correction), then we have

$$x = +0{\cdot}00131 \pm 0{\cdot}00094$$
$$y = +0{\cdot}00654 \pm 0{\cdot}00077.$$

Therefore, by making use of all the available stars observed at Melbourne during the period 1871–1875, we find, for stars south of the zenith, that the adopted refractions should be diminished by

$$0{\cdot}00474 \times \text{Refraction};$$

whilst from the observations of stars north of the zenith it appears that the adopted refractions should be diminished by

$$0{\cdot}00654 \times \text{Refraction}.$$

It is evident that, so far, we have no reason to conclude that the constant of refraction is different in different directions; as of course, from the nature of the case, if we assume that the factors are, or may be, different, we are sure to get results differing to some extent. In fact, Mr. Stone found differences fully as great as these in his discussion of the Greenwich refractions from North and South stars.

It may be remarked in passing that the value of $x$ found above indicates that if the correction had not been applied to the Greenwich thermometer readings, the resulting correction to the adopted refractions would have been practicably insensible. So that, as Mr. Christie has pointed out, as far as 80° Z.D. Bessel's Refractions accurately represent the Greenwich observations, when the thermometer reads about half a degree too high.

Suppose now that we reject all stars which pass the meridian at or below 85° Z.D. Then the normal equations derived from the observations of circumpolar stars become

$$380\,x + 67087\,y = 191{\cdot}93$$
$$67087\,x + 13652133\,y = 38305{\cdot}52.$$

From these equations I find

$$x = +0''.073 \quad \pm 0''.218$$
$$y = +0.00245 \pm 0.00115.$$

If $y$ were without error, the probable error of $x$ would be $\pm 0''.085$. The sum of squares of residuals, taking the weights into account, is 26·14.

We see, therefore, that the correction to the assumed co-latitude has changed sign, and the factor for correction to Bessel's Refractions is about half what it was before.

Proceeding in the same way with the North stars I find

$$6636687\,x + 2616843\,y + 54887\,\delta l = 34334{\cdot}71$$
$$2616843\,x + 14966138\,y + 79838\,\delta l = 112411{\cdot}46;$$

therefore

$$x = +0.00237 - 0.00662\,\delta l$$
$$y = +0.00710 - 0.00418\,\delta l.$$

The value of $y$ derived from the comparison with Greenwich therefore remains the same as when all the stars were used, provided the same value of $\delta l$ is substituted. If, however, we use the new value of $\delta l$, viz.

$$-0''.037 \pm 0''.109,$$

we find

$$y = +0.00725 \pm 0.00067.$$

The factors derived from North and South stars are now discordant to a considerable extent.

If we introduce into our normal equations only results deduced from stars passing the meridian above 82° Z.D. this discordance is still more strongly marked. Thus from the circumpolars we have

$$366\,x + 60465\,y = 171{\cdot}91$$
$$60465\,x + 10519927\,y = 28836{\cdot}06.$$

The solution of these equations gives

$$x = +0''.334 \quad \pm 0''.403$$
$$y = +0.00082 \pm 0.00221.$$

If $y$ be supposed to be without error, the probable error of $x$ becomes $\pm 0''.085$. The sum of squares of residuals is 22·87. The factor for correction of the adopted refractions is again much smaller than before; and, taking into account the magnitude of its probable error, practically unimportant.

For the North stars passing the meridian above 82° Z.D. we have

$$6635751\,x + 2547579\,y + 54731\,\delta l = 33927{\cdot}55$$
$$2547579\,x + 9840602\,y + 68294\,\delta l = 82281{\cdot}62\,;$$

whence

$$x = +0{\cdot}00211 - 0{\cdot}00642\,\delta l$$
$$y = +0{\cdot}00781 - 0{\cdot}00534\,\delta l\,;$$

or the value of $y$ is still the same, if $\delta l$ is the same. If

$$\delta l = -0''{\cdot}167 \pm 0''{\cdot}202$$

as found from circumpolar stars above 82° Z.D. at lower transit, then

$$y = +0{\cdot}00876 \pm 0{\cdot}00134.$$

The discordance between the values of $y$ North and South is therefore much increased.

If we proceed a step further and reject all stars below 77° Z.D. we find from the circumpolar stars actually a negative value for $y$; whilst for the North stars, compared with Greenwich the value still remains practically unaltered.

It appears, therefore, as the general result of this investigation, that when the very low stars are included, where of course the refraction comes in with greatest effect, we get a fair agreement between the corrections derived from North and from South stars; but as we go on omitting the lower stars, the discordance between the corresponding corrections on the North and South sides increases rapidly. It is evident, then, that there is a serious systematic error affecting the observations made on one side or other of the zenith. There is, however, no reason to suppose that this arises from a difference in the mean refractions, resulting from the circumstance of the meridian on one side being over the sea and on the other side over the land, and the atmosphere therefore being in a different state with regard to moisture, as has been supposed; for, *if this* were the true explanation, the discordance referred to ought at all events not to *diminish* as the lower stars are included, whereas I have shown that it diminishes rapidly until at last it becomes practically insignificant. The origin of the discordance is more probably some mechanical defect in the instrument, as Mr. Christie has suggested.

I may remark that in Mr. Stone's discussion of the Melbourne results 1863–1865 referred to above, all the stars used are above 82° Z.D., so that the discordance which he has found is borne out to a great extent by the results of this investigation.

It becomes now a question of interest to determine, as far as possible, on which side of the zenith the Melbourne N.P.D.'s are most correct. Although no great certainty can be arrived at

with the means of judging the question which are available to us, still I think it most probable that it is the stars on the north side of the zenith which are most affected by systematic error. For it will be remarked that the circumpolar stars give very much the same results as Main's exhaustive discussion gave for the Greenwich N.P.D.'s—that is, that Bessel's Refractions unaltered answer very well down to 82° Z.D.; from 82° to 85° Z.D. that the refractions of the *Tabulæ* appeared to require to be diminished by ·00230 × Refraction; and below 85° Z.D. that the refractions of the *Fundamenta* should be used diminished by ·00152 × Refraction, the refractions of the *Fundamenta* being equivalent to those of the *Tabulæ* diminished by ·00327 × Refraction of the *Tabulæ*. These results agree very closely with those found in this paper for the Melbourne circumpolar stars, and therefore I conclude that it is probable that the N.P.D.'s of these stars are substantially correct. And in fact the substitution of Main's corrections below 82° Z.D. gives smaller residuals than I have obtained from the solution by the method of least squares. For, applying these corrections and putting $y=0$, we get

$$x = +0''.474 \pm 0''.065,$$

and the sum of squares of residuals (the weights being taken into account) is only 24·96: in the former case it was 44·43.

If we attempt to determine the co-latitude from observations of circumpolar stars which pass the meridian above 82° Z.D. at the lower transit, assuming that the refractions of the *Tabulæ* are correct down to that limit, we find

$$x = +0''.470 \pm 0''.077,$$

and the sum of squares of residuals is 23·54: in the solution given above for these stars this quantity comes out 22·87, so that our assumption is satisfactorily borne out, considering the difficulty of separating errors of co-latitude and of refraction at the latitude of Melbourne. I should recommend therefore that, for the present, a correction of $+0''.20$ be applied to the co-latitude assumed in this volume, thus making

$$\text{Co-latitude} = 52^{\circ}\ 10'\ 6''.8;$$

and that the refractions of Bessel's *Tabulæ* be used unaltered, at all events down to 82° Z.D.

I understand that efforts are being made to obtain a new Transit Circle, more suited to the requirements of modern astronomy, for the Melbourne Observatory. Until we have a series of observations made with such an instrument, I do not think that we can approximate more closely either to the true value of the latitude, or to the value of the constant of refraction

required by the Melbourne observations, than I have done in the present paper.

I add here, for convenience of reference, the separate comparisons, both for circumpolars and for stars compared with the Greenwich Catalogue, of all stars which pass the meridian of Melbourne below 82° Z.D. observed in the years 1871–1875.

I. *Circumpolar Stars.*

| Star's Name. | N.P.D. | Upper−Lower. | Number of Obs. Upper. | Lower. |
|---|---|---|---|---|
| | ° ′ | ″ | | |
| β Centauri | 149 46 | −0·06 | 7 | 5 |
| ε Argûs | 149 6 | +2·18 | 7 | 6 |
| η Argûs | 149 1 | +1·86 | 12 | 4 |
| ι Argûs | 148 45 | +1·96 | 8 | 7 |
| α Eridani | 147 53 | +1·14 | 6 | 11 |
| α Pavonis | 147 8 | +3·19 | 10 | 9 |
| ζ Aræ | 145 47 | +3·38 | 6 | |
| α Doradûs | 145 18 | +5·10 | 7 | 5 |

II. *North Stars.*

| Star' Name. | N.P.D. | Greenwich−Melbourne. | Number of Obs. Greenwich. | Melbourne. |
|---|---|---|---|---|
| | ° ′ | ″ | | |
| δ Cygni | 45 11 | +1·91 | 19 | 5 |
| α Cygni | 45 11 | +1·95 | 48 | 13 |
| ξ Andromedæ | 45 8 | +5·28 | 3 | 3 |
| β Aurigæ | 45 4 | +0·77 | 16 | 6 |
| ψ Ursæ Majoris | 44 48 | +1·11 | 13 | 6 |
| α Aurigæ | 44 8 | +3·12 | 45 | 12 |
| τ Herculis | 43 23 | +0·46 | 23 | 2 |
| δ Persei | 42 37 | +2·55 | 5 | 5 |
| 51 Andromedæ | 42 1 | (+10·50) | 6 | 3 |
| ι Ursæ Majoris | 41 27 | +3·04 | 17 | 10 |
| α Persei | 40 36 | +6·73 | 37 | 7 |
| η Ursæ Majoris | 40 3 | (+12·81) | 77 | 4 |

The quantities enclosed within brackets have not been used in the present investigation. The above comparison is made with the Greenwich places as printed in the Nine-Year Catalogue.

*Transit Times of the Spots on Jupiter.* By W. F. Denning, Esq.

During the past autumn and winter a number of observations of *Jupiter* were made here* for the purpose of estimating the times of transit of the Red Spot and of other conspicuous objects on his disk. The telescope was a Browning-With Reflector of 10 inches' aperture, power 300, and a Barlow lens, which increased it to 450, was invariably used. The results are given in a tabulated form for each object separately.

*Transits of the Great Red Spot.*

| | | Preceding End. | Middle. | Following End. | Middle of Spot. Precedes first Meridian. |
|---|---|---|---|---|---|
| | | h m | h m | h m | m |
| 1880, Sept. | 27 | 10 26 | 10 51 | 11 15 | 54·9 |
| | 30 | 7 56 | 8 16 | 8 39 | 57·4 |
| Oct. | 11 | 11 47 | 12 16 | 12 46 | 52·1 |
| | 14 | 9 17 | 9 46 | 10 12 | 49·7 |
| | 18 | 12 39 | 13 5 | 13 35 | 44·5 |
| | 23 | 11 44 | 12 12 | 12 41 | 42·1 |
| | 24 | 7 34 | 7 59 | 8 26 | 45·9 |
| | 29 | 6 40 | 7 8 | 7 36 | 41·7 |
| Nov. | 2 | 10 0 | 10 26 | 10 57 | 37·8 |
| | 3 | 5 44 | 6 12 | | 42·7 |
| | 4 | 11 36 | 12 4 | 12 38 | 36·9 |
| | 8 | 4 50 | 5 21 | 5 53 | 38·8 |
| | 9 | 10 41 | 11 12 | 11 39 | 34·1 |
| | 14 | | 10 20 | 10 45 | 31·4 |
| | 17 | 7 17 | 7 44 | 8 12 | 35·6 |
| | 19 | 8 59 | 9 23 | 9 52 | 33·9 |
| | 20 | 4 48 | 5 14 | 5 43 | 33·8 |
| | 21 | 10 38 | 11 4 | 11 32 | 30·3 |
| | 25 | | 4 25 | 4 49 | 28 6 |
| | 27 | 5 34 | 6 0 | 6 29 | 31·0 |
| | 28 | 11 24 | 11 49 | | 28·5 |
| | 29 | 7 11 | 7 40 | 8 9 | 28·4 |
| Dec. | 2 | 4 38 | 5 8 | 5 37 | 28·9 |
| | 10 | 11 20 | 11 47 | | 20·2 |

* An abstract of my earlier observations appears in the *Monthly Notices* for November, 1880, pp. 44-46.

| | Preceding End. | Middle. | Following End. | Middle of Spot. Precedes first Meridian. |
|---|---|---|---|---|
| | h m | h m | h m | m |
| 1880, Dec. 15 | 10 30 | 10 56 | 11 22 | 17·5 |
| 20 | 9 38 | 10 4 | 10 33 | 16·0 |
| 25 | 8 40 | 9 12 | 9 39 | 14·6 |
| 28 | 6 15 | 6 40 | 7 7 | 15·5 |
| 30 | 7 50 | 8 21 | 8 50 | 12·4 |
| 31 | | 4 11 | 4 39 | 13·5 |
| 1881, Jan. 6 | 8 40 | 9 7 | 9 33 | 11·1 |
| 7 | 4 31 | 4 57 | 5 25 | 12·3 |
| 12 | | 4 5 | 4 32 | 11·3 |
| 13 | 9 27 | | | |
| 14 | 5 17 | 5 44 | 6 13 | 10·3 |
| 16 | 6 54 | 7 23 | 7 52 | 9·2 |
| 31 | | 4 49 | 5 18 | 4·9 |
| March 17 | 6 36 | 7 1 | | +3·5 |

Average duration of transit=55·5 min. (28 transits).

On December 2, 1880, the following end of the spot had the same longitude as the assumed first meridian of *Jupiter* computed by Mr. Marth* from the rotation period of spots seen in 1834–5 and 1862—namely, $9^h\ 55^m\ 27^s{\cdot}08$, which corresponds to a daily rate of 870°·600. The red spot moves slower than this, its period being as nearly as possible 7 seconds greater than that adopted in calculating the passages of the first meridian. Thus during the period embraced by the above observations the comparative times of transit of the spot and of the assumed first meridian shows a considerable difference, for while on Sept. 27, 1880, the middle of the spot preceded this element by 55 minutes, it had so far lost ground by March 17, 1881, as to follow it by $3\frac{1}{2}$ minutes; the difference amounting to nearly an hour in the 171 days. The centre of the spot must have had coincident longitude with the assumed first meridian on about February 26, but no observations could be made at that epoch owing to cloudy weather.

In October I frequently noticed a dusky elongated marking or short streak south preceding the red spot about $1\frac{1}{2}$ hour. It crossed the central meridian of *Jupiter* at about $10^h\ 35^m$ on Oct. 11 and at the end of that month I began to note it particularly. The following are the times when the centre or following end was estimated as central :—

* See *English Mechanic*, Nos. 807, 814, and 821.

*Transits of Dusky Streak s.p. Red Spot.*

| | Middle. | Following End. | Middle Precedes ♑'s First Meridian. | Precedes Red Spot. |
|---|---|---|---|---|
| | h m | h m | h m | h m |
| 1880, Oct. 11 | 10 35? | | | 1 41? |
| 31 | 7 15 | | 2 11·8 | |
| Nov. 2 | 8 48 | 8 59 | 2 15 8 | 1 38 |
| 4 | 10 28 | 10 37 | 2 12·9 | 1 36 |
| 8 | 13 41 | 13 53 | 2 14·2 | |
| 12 | 6 59 | | 2 15·2 | |
| 17 | 6 3 | 6 15 | 2 16·6 | 1 41 |
| 22 | 5 7 | | 2 18·2 | |
| 24 | 6 46 | | 2 16·6 | |
| 26 | 8 25 | | 2 15·0 | |
| 27 | 4 16 | | 2 15·0 | 1 44 |
| 28 | 10 1 | 10 16 | 2 16·5 | 1 48 |
| 29 | 5 48 | 6 5 | 2 20·4 | 1 52 |
| Dec. 18 | | 6 38 | | |
| 20 | 7 56 | 8 12 | 2 24·0 | 2 8 |
| 21 | | 4 11 | | |
| 25 | 6 59 | 7 14 | 2 27·6 | 2 13 |
| 28 | | 4 44 | | |
| 30 | 6 3 | 6 20 | 2 30·4 | 2 18 |
| 1881, Jan. 6 | 6 45 | 6 59 | 2 33·1 | 2 22 |
| 8 | 8 22 | 8 40 | 2 34·0 | |
| 13 | 7 24 | 7 43 | 2 39·1 | |
| 16 | 4 52 | 5 8 | 2 40·2 | 2 31 |
| Feb. 16 | | 5 35 | | |

The first observation on Oct. 11 is doubtful; the time is probably too early.

Average duration of transit (computed from 13 observed passages of the middle and following end of the spot) 29·4 min., so that the spot was slightly more than half the length of the red spot. When definition was poor it appeared more diffused and longer than when the image was sharp and the spot seen in a favourable and true aspect. Mr. Barnard, at Nashville, Tennessee, had figured this spot (*English Mechanic*, No. 804) as early as July 25, 1880, when it was apparently attached to the preceding end of the red spot; but it had subsequently been rotating in shorter intervals than that object, and its swifter motion had caused its gradual drift westwards. In the above table I have compared the period as observed here by which

it preceded *Jupiter's* first meridian and the red spot, and it will be noticed that while on November 2 and 4 it came to transit slightly more than $1\frac{1}{2}$ hour before the red spot, the interval had increased to $2\frac{1}{2}$ hours by January 16. In fine, its period of rotation was performed in $17\frac{1}{2}$ seconds less time than the red spot,* and this accords exactly with similar observations by Mr. Barnard, who has been watching another object of the same character in the southern hemisphere of *Jupiter*, and deduced a period of $9^h\ 55^m\ 16^s{\cdot}176$.† There were a number of these dusky short streaks visible in this region of the planet during his recent opposition; they were markedly persistent, and, though preserving the same positions relatively to each other, they all moved quicker than the red spot.

On September 18, 1880, I detected a very brilliant spot just south of the Jovian equator, and the same object was observed on many subsequent occasions. After the middle of October I obtained as many of its transits as possible, for it gave the most unmistakeable signs of great proper motion. The same feature was frequently observed by Schmidt at Athens in October and November 1880 (see *Astronomische Nachrichten*, No. 2353), and he has deduced its rotation period as $9^h\ 50^m$, or $5^m\ 34^s$ less than the red spot.

The following are the transits observed at Bristol :—

*Bright Spot South of Equator.*

| | h m | | h m |
|---|---|---|---|
| 1880, Sept. 18 | 11 50? | 1880, Nov. 29 | 5 27 |
| Oct. 20 | 11 38 | Dec. 3 | 7 54 |
| 30 | 7 28 | 10 | 7 14 |
| Nov. 2 | 14 15 | 12 | 8 27 |
| 3 | 9 47 | 15 | 5 16 |
| 4 | 5 25 | 21 | 8 49 |
| 8 | 7 48 | 23 | 10 4 |
| 17 | 8 14 | 28 | 7 59 |
| 19 | 9 23 | 1881, Jan. 6 | 8 27 |
| 20 | 5 2 | 7 | 4 5 |
| 21 | 10 41 | 13 | 7 37 |
| 26 | 8 44 | 16 | 4 28 |
| 27 | 4 19 | Feb. 23 | 7 5? |
| 28 | 9 51 | | |

* See *English Mechanic*, No. 824, Jan. 7, 1881, p. 421.

† The small spot referred to was mentioned in *Monthly Notices*, Nov. 1880, p. 46, as "maintaining a permanent position with reference to the red spot," but this was stated before my prolonged observations had enabled me to detect the differences here referred to.

I gave the period as $9^h\ 50^m\ 5^s$ in *English Mechanic*, No. 818, Nov. 26, p. 277,* and this agrees with that derived by Mr. Marth (*ibid.* No. 830, Feb. 18, p. 564) from more comprehensive observations during the interval Sept. 18, 1880–Jan. 13, 1881, by Schmidt, Corder, Dennett, McCance, Barnard, and myself, including 55 transits which indicated the daily rate of rotation as 878°·48, and the corresponding period $9^h\ 50^m\ 6^s{\cdot}6$. This remarkable spot was noticed by many observers independently. It came to conjunction with the red spot on Nov. 19 and Jan. 3, but on the latter date clouds prevented observation. After Jan. 16, a prolonged period of overcast skies prevented a continuance of the observations, and the spot was not identified afterwards, though on Feb. 23, at $7^h\ 5^m$ I saw a bright object nearly in the calculated place, and it was watched in transit. Its absolute identity with the spot so frequently seen during Oct., Nov., and Dec., could not, however, be inferred, and there is also some doubt attached to the original observation on Sept. 18, owing to the long intervening period between that date and the next observed transit on Oct. 20. There were several of these bright spots presented on *Jupiter* in very nearly the same latitude, so that, unless the observations were made at short intervals, there was considerable difficulty in distinguishing them apart. But, fortunately, in the case of the brilliant object I have been particularly alluding to, it was of special type, being considerably brighter and more conspicuous than any others of its class, and situated slightly further south. It was occasionally noted as extremely faint, while at other times it seemed very distinct and comparable to the bright disk of a satellite on the limb of *Jupiter*, near the beginning or end of a transit. Generally, however, it was very distinct, and its rapid motion relatively to the red spot could be clearly traced on succeeding nights. It was preceded and followed closely by spots of similar character, though fainter, and intermittently visible, which showed the same swift movement westwards; indeed, the mass of detail manifested on the equator appeared to be influenced by the same rapid current relatively to the red spot, and by some singular variations in form. Many large white patches made their appearance, but soon became diffused along the light N. border of the great South belt, and there were dusky strips intervening which also participated in the remarkable motion and variations referred to.

The small dark spots visible in a belt N. of the great equatorial zone, which I referred to in my paper in *Monthly Notices* for Nov. 1880, pp. 44–46, were re-observed as frequently as possible, but their variations of position and form were so great as to render it almost impossible to trace the identical objects at their succeeding apparitions. The two chief spots of the cluster were, however, followed as long as practicable, and the following transits observed here:—

* See also *Monthly Notices* for Nov. 1880, p. 45.

| | | I. h m | II. h m | | | I. h m | II. h m |
|---|---|---|---|---|---|---|---|
| 1880, Oct. | 29 | 7 31 | 7 54 | 1880, Dec. | 2 | 4 55 | 5 39 |
| Nov. | 2 | 9 31 | 9 52 | | 15 | | 6 48 |
| | 3 | 5 5 | 5 31 | | 20 | | 4 11 |
| | 4 | 10 41 | 11 4 | 1881, Jan. | 6 | 7 30 | 8 11 |
| | 8 | 12 37 | 13 3 | | 7 | | 3 50 |
| | 23 | 5 14 | 5 39 | | 8 | 8 46 | |
| | 29 | | 8 44 | | | | |

These spots travelled with greater velocity than any other of the markings. The approximate period was $9^h\ 48^m$, but the motion was not uniform. They were observed to overtake and pass the white spot on the equator early in January.

Towards the middle of January they apparently formed a complete girdle around the planet, but the spots were evidently losing their distinctive character. What had formerly been a single belt N. of the great equatorial zone was now a double belt; the one nearest the equator was the most conspicuous, and had evidently been formed by the dispersion of the materials of the spots, which had been going on under the eyes of observers during the three preceding months.

## *Ephemeris for Physical Observations of Jupiter*, 1881–82. By A. Marth, Esq.

| Greenwich Noon. | Angle of Position of ♃'s Axis. | Latitude of Earth above ♃'s Equator. | Latitude of Sun above ♃'s Equator. | Annual Parallax A−L. | O − L. | Longitude of ♃'s Central Meridian. | Diff. | Corr. for Phase. |
|---|---|---|---|---|---|---|---|---|
| 1881, June 28 | 343°·087 | +3°·058 | +3°·050 | −8°·929 | 87°·612 | 96°·05 | 4351°·44 | +0°·35 |
| July 3 | 343·405 | 3·076 | 3·053 | 9·423 | 86·662 | 127·49 | 1·50 | ·39 |
| 8 | 343·714 | 3·093 | 3·055 | 9·881 | 85·749 | 158·99 | 1·56 | ·43 |
| 13 | 344·013 | 3·110 | 3·057 | 10·297 | 84·878 | 190·55 | 1·61 | ·46 |
| 18 | 344·301 | 3·127 | 3·059 | 10·667 | 84·053 | 222·16 | 1·67 | ·50 |
| 23 | 344·575 | 3·143 | 3·061 | 10·989 | 83·277 | 253·83 | 1·73 | ·53 |
| 28 | 344·834 | +3·159 | +3·063 | −11·259 | 82·553 | 285·56 | 1·80 | + ·55 |
| Aug. 2 | 345·075 | 3·175 | 3·065 | 11·473 | 81·885 | 317·36 | 1·87 | ·57 |
| 7 | 345·297 | 3·190 | 3·066 | 11·628 | 81·276 | 349·23 | 1·93 | ·59 |
| 12 | 345·498 | 3·206 | 3·067 | 11·720 | 80·730 | 21·16 | 2·00 | ·60 |
| 17 | 345·675 | 3·221 | 3·068 | 11·745 | 80·251 | 53·16 | 2·07 | ·60 |
| 22 | 345·828 | 3·235 | 3·069 | 11·700 | 79·842 | 85·23 | 2·15 | ·59 |
| 27 | 345·954 | +3·249 | +3·070 | −11·581 | 79·507 | 117·38 | 2·22 | + ·58 |
| Sept. 1 | 346·051 | 3·263 | 3·070 | 11·385 | 79·249 | 149·60 | 2·28 | ·56 |
| 6 | 346·119 | 3·276 | 3·070 | 11·110 | 79·071 | 181·88 | 2·36 | ·54 |
| 11 | 346·157 | 3·289 | 3·070 | 10·754 | 78·974 | 214·24 | 2·43 | ·50 |
| 16 | 346·163 | 3·301 | 3·070 | 10·315 | 78·961 | 246·67 | 2·50 | ·46 |
| 21 | 346·137 | 3·311 | 3·069 | 9·791 | 79·032 | 279·17 | 2·57 | ·42 |

| Greenwich Noon. | Angle of Position of ♃'s Axis. | Latitude of Earth above ♃'s Equator. | Latitude of Sun above ♃'s Equator. | Annual Parallax A—L. | O—L. | Longitude of ♃'s Central Meridian. | Diff. | Corr. for Phase. |
|---|---|---|---|---|---|---|---|---|
| | ° | ° | ° | ° | ° | ° | ° | ° |
| 1881, Sept. 26 | 346·080 | +3·320 | +3·069 | −9·184 | 79·186 | 311·74 | 2·63 | +·37 |
| Oct. 1 | 345·992 | 3·327 | 3·068 | 8·495 | 79·423 | 344·37 | 2·68 | ·31 |
| 6 | 345·874 | 3·333 | 3·067 | 7·726 | 79·740 | 17·05 | 2·73 | ·26 |
| 11 | 345·729 | 3·337 | 3·066 | 6·882 | 80·132 | 49·78 | 2·77 | ·21 |
| 16 | 345·559 | 3·338 | 3·065 | 5·968 | 80·595 | 82·55 | 2·80 | ·16 |
| 21 | 345·366 | 3·337 | 3·063 | 4·990 | 81·122 | 115·35 | 2·82 | ·11 |
| 26 | 345·155 | +3·333 | +3·061 | −3·958 | 81·702 | 148·17 | 2·83 | +·07 |
| 31 | 344·930 | 3·326 | 3·059 | 2·884 | 82·325 | 181·00 | 2·83 | ·04 |
| Nov. 5 | 344·696 | 3·317 | 3·057 | 1·778 | 82·980 | 213·83 | 2·81 | +·01 |
| 10 | 344·456 | 3·304 | 3·055 | −0·652 | 83·655 | 246·64 | 2·77 | ·00 |
| 15 | 344·216 | 3·289 | 3·053 | +0·482 | 84·338 | 279·41 | 2·73 | ·00 |
| 20 | 343·981 | 3·271 | 3·050 | 1·608 | 85·015 | 312·14 | 2·67 | −·01 |
| 25 | 343·754 | +3·250 | +3·047 | +2·714 | 85·672 | 344·81 | 2·60 | −·03 |
| 30 | 343·541 | 3·228 | 3·044 | 3·787 | 86·296 | 17·41 | 2·52 | ·06 |
| Dec. 5 | 343·345 | 3·204 | 3·041 | 4·817 | 86·875 | 49·93 | 2·43 | ·10 |
| 10 | 343·169 | 3·179 | 3·037 | 5·792 | 87·400 | 82·36 | 2·33 | ·15 |
| 15 | 343·015 | 3·152 | 3·034 | 6·702 | 87·862 | 114·69 | 2·23 | ·20 |
| 20 | 342·886 | 3·125 | 3·030 | 7·541 | 88·252 | 146·92 | 2·12 | ·25 |

| Greenwich Noon. | Angle of Position of ♃'s Axis. | Latitude of Earth above ♃'s Equator. | Latitude of Sun above ♃'s Equator. | Annual Parallax A−L. | O−L. | Longitude of ♃'s Central Meridian. | Diff. | Corr. for Phase. |
|---|---|---|---|---|---|---|---|---|
| | ° | ° | ° | ° | ° | ° | ° | ° |
| 1881, Dec. 25 | 342·782 | +3·099 | +3·026 | +8·302 | 88·564 | 179·04 | 2·02 | −·30 |
| 30 | 342·705 | 3·073 | 3·022 | 8·980 | 88·793 | 211·06 | 1·91 | ·35 |
| 1882, Jan. 4 | 342·656 | 3·047 | 3·018 | 9·573 | 88·938 | 242·97 | 1·80 | ·40 |
| 9 | 342·635 | 3·022 | 3·013 | 10·079 | 88·997 | 274·77 | 1·70 | ·44 |
| 14 | 342·642 | 2·998 | 3·008 | 10·499 | 88·968 | 306·47 | 1·61 | ·48 |
| 19 | 342·677 | 2·976 | 3·003 | 10·832 | 88·853 | 338·08 | 1·51 | ·51 |
| 24 | 342·740 | +2·955 | +2·998 | +11·080 | 88·655 | 9·59 | 1·42 | −·53 |
| 29 | 342·830 | 2·936 | 2·993 | 11·246 | 88·375 | 41·01 | 1·33 | ·55 |
| Feb. 3 | 342·946 | 2·918 | 2·988 | 11·333 | 88·015 | 72·34 | 1·26 | ·56 |
| 8 | 343·087 | 2·901 | 2·982 | 11·344 | 87·580 | 103·60 | 1·19 | ·56 |
| 13 | 343·253 | 2·885 | 2·976 | 11·284 | 87·073 | 134·79 | 1·12 | ·55 |
| 18 | 343·444 | 2·871 | 2·970 | 11·156 | 86·498 | 165·91 | 1·07 | ·54 |
| 23 | 343·659 | +2·858 | +2·964 | +10·963 | 85·859 | 196·98 | 1·02 | −·52 |
| 28 | 343·897 | 2·847 | 2·958 | 10·710 | 85·160 | 228·00 | 0·97 | ·50 |
| March 5 | 344·156 | 2·836 | 2·952 | 10·401 | 84·406 | 258·97 | 0·93 | ·47 |
| 10 | 344·436 | 2·826 | 2·945 | 10·042 | 83·602 | 289·90 | 0·89 | ·44 |
| 15 | 344·737 | 2·817 | 2·938 | 9·635 | 82·750 | 320·79 | 0·87 | ·40 |
| 20 | 345·058 | 2·809 | 2·931 | 9·183 | 81·854 | 351·66 | 4350·85 | ·37 |
| 25 | 345·398 | +2·802 | +2·924 | +8·691 | 80·918 | 22·51 | | −0·33 |

A − L is the difference of the Jovicentric longitudes of the Sun and the Earth, reckoned in the plane of *Jupiter's* equator, O − L the difference of longitudes of *Jupiter's* vernal equinoctial point O and of the point L of his celestial equator, which is in opposition to the Earth. If the "Corr. for phase" in the last column is applied to the "Longitude of ♃'s central meridian," or of the meridian directed to the Earth, the longitude of the meridian is found which bisects the illuminated disk of *Jupiter*.

The daily rate of rotation adopted in the present ephemeris is 870°·42, the corresponding period being $9^h\ 55^m\ 34^s{\cdot}47$. This rate represents very nearly the average motion of the great reddish spot between the last months of 1878 and the first months of 1881. The observations show that during this interval the motion of the spot has slackened, and the rate of retardation seems to be increasing. In the ephemerides, published in the *Monthly Notices*, for the last six apparitions of *Jupiter*, the adopted First Meridian was assumed to rotate at the daily rate 870°·60. While at the time of the early observations in 1878 the middle of the great spot preceded the First Meridian by nearly 160°, this interval continually decreased by the difference of their motions, and towards the end of February 1881 the First Meridian has overtaken the middle of the spot. On account of the obvious advantage of keeping it conveniently near the middle, it appeared expedient to introduce the altered rate of rotation and to fix upon Feb. 28·0 as the time when the computed longitudes of the central meridian should be the same in the altered system as in the former one, so that the correction—

$$+0^\circ{\cdot}18\ (1881,\ \text{Feb.}\ 18{\cdot}0 - t)$$

must be applied to the longitudes of the former ephemerides, in order to reduce them to the system adopted in the present ephemeris. This correction is for

| | | | | | |
|---|---|---|---|---|---|
| 1875, Feb. 20 | + 36°·0 | 1877, April 30 | + 252°·0 | 1879, July 9 | + 108°·0 |
| May 31 | 18·0 | Aug. 8 | 234·0 | Oct. 17 | 90·0 |
| 1876, May 15 | 315·0 | Nov. 16 | 216·0 | 1880, Jan. 25 | 72·0 |
| July 4 | 306·0 | 1878, June 4 | 180·0 | Aug. 12 | 36·0 |
| Aug. 23 | 297·0 | Sept. 12 | 162·0 | Nov. 20 | 18·0 |
| Oct. 12 | + 288·0 | Dec. 21 | + 144·0 | 1881, Feb. 18 | + 0·0 |

The corrected longitudes corresponding to the recorded times of the observed passages of the great spot across the middle meridian may then serve as the foundation for a thorough investigation of its motions; but I defer the publication of a list of observed passages and their corresponding longitudes till it can

be given with some completeness. A list of the sketches of *Jupiter* in possession of the Royal Astronomical Society, arranged separately for each apparition of the planet, according to the longitudes of their middle meridian, must also be deferred. For the present, I content myself with supplementing Prof. Pritchett's communication at the end of this paper, containing his observations of the passages of the red spot, made during the last apparition of *Jupiter* at the Morrison Observatory, Glasgow, Missouri, by adding the corresponding longitudes. The evidence on the motion of the spot, furnished by the partial lists of observed passages, published in the *Monthly Notices*, vol. xl. pp. 420–428, and p. 496, and in the *English Mechanic* of Oct. 29, Dec. 17, and Feb. 18 last, or vol. xxxii. pp. 182, 350, and 563, may be rendered available by adding to the "Corr. Longitudes" there given under the heading "Rot. 870°·43" the quantity

| | | | |
|---|---|---|---|
| 1878, Sept. 12 | +94°·0 | 1880, Jan. 25 | +89°·0 |
| Dec. 21 | 93·0 | Aug. 12 | 87·0 |
| 1879, July 9 | 91·0 | Nov. 20 | 86·0 |
| Oct. 17 | +90·0 | 1881, Feb. 28 | +85·0 |

which will reduce them to the system of the present ephemeris.

The only passages, observed in 1878, which have yet become publicly known, are those of Mr. Trouvelot. The observations were kindly communicated to me by letter, and the times, published on p. 496 of vol. xl., are in accordance with the letter, excepting the hour on Dec. 16, which should be 4 instead of 6, the "corresponding longitude" belonging to the correct time $4^h\,35^m$. Mr. Trouvelot has now published a general account of his "Observations on *Jupiter*" in the "Proceedings of the American Academy of Arts and Sciences," and a copy of his interesting paper has reached me just in time for the present paper. In the list of observed passages of the great spot on page 307, the time of passage

| | h | m | | h | m | |
|---|---|---|---|---|---|---|
| 1878, Sept. 30 is given | 6 | 5 | instead of | 6 | 35 | in the letter. |
| Oct. 2 | 7 | 5 | | 7 | 45 | |
| Dec. 16 | 4 | 40 | | 4 | 35 | |

and, moreover, the time of passage of the centre on Sept. 25 is recorded. But though on page 308, where the discrepancy is pointed out, it is expressly stated that the time recorded in the paper is right, it is a question whether the correction for Oct. 2 is not in error. I now give the list of observed passages of 1878,

together with the corresponding longitudes of the middle meridian according to the system adopted in the present ephemeris.

| | Observed Passages. Mean time Cambridge, Mass. | | | Corresponding Longitudes. | | |
|---|---|---|---|---|---|---|
| | Prec. End. | Middle. | Foll. End. | Prec. End. | Middle. | Foll. End. |
| | h m | h m | h m | ° | ° | ° |
| 1878, Sept. 25 | | 6 55 | | | 0·6 | |
| 30 | | 6 5 | | | 2·0 | |
| Oct. 2 | | ? { 7 5 / 7 45 | | | 338·9 / 3·1 | ? |
| Nov. 12 | | | 6 50 | | | 10·2 |
| 24 | 6 10 | | | 348 4 | | |
| 29 | 5 15 | | | 346·1 | | |
| Dec. 16 | | 4 40 | | | 358·0 | |
| 23 | | 5 33 | | | 1·2 | |
| 28 | | 4 45 | | | 3·0 | |

The following table gives the values of the Equatoreal Diameter of *Jupiter* and "Def." its defect of illumination, of the Polar Diameter, the defect of which is insensible, and of the differences of limbs in right ascension and declination, with their defects of illumination, together with the auxiliary angles $d$ and $w$, required in the computation, as explained in the "Addition" to last year's ephemeris, vol. xl. p. 490 ff. :—

| Greenwich Noon. | Equatoreal Diameter. | Def. prec. | Polar Diameter. | Difference between Limbs in A.R. | Def. prec. l. | in Decl. | Def. south l. | $d$. | $w$. |
|---|---|---|---|---|---|---|---|---|---|
| | ″ | ″ | ″ | s | s | ′ | ″ | ° | ° |
| 1881, June 28 | 35·10 | 0·21 | 32·87 | 2·423 | 0·014 | 33·06 | 0·02 | 8·91 | 269·80 |
| July 3 | 35·47 | ·24 | 33·22 | 2·453 | ·015 | 33·41 | ·02 | 9·40 | 269·88 |
| 8 | 35·87 | ·27 | 33·60 | 2·484 | ·017 | 33·78 | ·02 | 9·86 | 269·95 |
| 13 | 36·30 | ·29 | 34·00 | 2·517 | ·019 | 34·18 | ·02 | 10·28 | 270·01 |
| 18 | 36·76 | ·32 | 34·43 | 2·552 | ·021 | 34·61 | ·02 | 10·65 | 270·07 |
| 23 | 37·25 | ·34 | 34·89 | 2·589 | ·022 | 35·06 | ·02 | 10·97 | 270·13 |
| 28 | 37·77 | ·36 | 35·37 | 2·627 | ·024 | 35·54 | ·03 | 11·24 | 270·19 |
| Aug. 2 | 38·31 | ·38 | 35·88 | 2·668 | ·026 | 36·04 | ·03 | 11·46 | 270·25 |
| 7 | 38·88 | ·40 | 36·41 | 2·710 | ·026 | 36·57 | ·03 | 11·61 | 270·31 |
| 12 | 39·47 | ·41 | 36·96 | 2·753 | ·027 | 37·12 | ·03 | 11·70 | 270·37 |
| 17 | 40·08 | ·42 | 37·54 | 2·798 | ·028 | 37·70 | ·03 | 11·73 | 270·44 |
| 22 | 40·72 | ·42 | 38·13 | 2·844 | ·028 | 38·29 | ·02 | 11·68 | 270·52 |
| 27 | 41·37 | ·42 | 38·74 | 2·891 | ·028 | 38·90 | ·02 | 11·56 | 270·60 |
| Sept. 1 | 42·03 | ·41 | 39·36 | 2·938 | ·027 | 39·52 | ·02 | 11·37 | 270·70 |
| 6 | 42·70 | ·40 | 39·99 | 2·986 | ·027 | 40·15 | ·02 | 11·09 | 270·81 |
| 11 | 43·38 | ·38 | 40·62 | 3·033 | ·025 | 40·79 | ·02 | 10·74 | 270·93 |
| 16 | 44·05 | ·35 | 41·25 | 3·080 | ·024 | 41·42 | ·02 | 10·30 | 271·06 |
| 21 | 44·71 | ·32 | 41·87 | 3·126 | ·022 | 42·04 | ·02 | 9·78 | 271·21 |
| 26 | 45·35 | ·29 | 42·47 | 3·170 | ·019 | 42·64 | ·01 | 9·17 | 271·40 |

| Greenwich Noon. | Equatoreal Diameter. | Def. prec. | Polar Diameter. | Difference between Limbs in A.R. | Def. prec. l. | in Decl. | Def. south l. | $d$. | $w$. |
|---|---|---|---|---|---|---|---|---|---|
| | ″ | ″ | ″ | s | s | ″ | ″ | ° | ° |
| 1881, Oct. 1 | 45·97 | ·25 | 43·05 | 3·211 | ·017 | 43·22 | ·01 | 8·48 | 271·62 |
| 6 | 46·54 | ·21 | 43·59 | 3·250 | ·014 | 43·77 | ·01 | 7·72 | 271·87 |
| 11 | 47·08 | ·17 | 44·09 | 3·285 | ·011 | 44·28 | ·01 | 6·88 | 272·19 |
| 16 | 47·55 | ·13 | 44·54 | 3·316 | ·009 | 44·73 | ·01 | 5·96 | 272·62 |
| 21 | 47·96 | ·09 | 44·92 | 3·342 | ·006 | 45·12 | ·00 | 4·99 | 273·21 |
| 26 | 48·30 | ·06 | 45·24 | 3·362 | ·004 | 45·44 | | 3·96 | 274·09 |
| 31 | 48·56 | ·03 | 45·48 | 3·376 | ·002 | 45·69 | | 2·89 | 275·58 |
| Nov. 5 | 48·72 | ·01 | 45·63 | 3·385 | ·001 | 45·85 | | 1·80 | 278·84 |
| 10 | 48·80 | ·01 | 45·70 | 3·387 | ·000 | 45·93 | | 0·70 | 292·24 |
| 15 | 48·78 | foll. l. | 45·68 | 3·382 | foll. l. | 45·92 | | 0·54 | 62·26 |
| 20 | 48·66 | ·01 | 45·57 | 3·370 | ·001 | 45·82 | | 1·62 | 81·69 |
| 25 | 48·45 | ·03 | 45·38 | 3·352 | ·002 | 45·63 | north l. | 2·72 | 85·50 |
| 30 | 48·16 | ·05 | 45·10 | 3·329 | ·003 | 45·35 | ·00 | 3·79 | 87·14 |
| Dec. 5 | 47·78 | ·08 | 44·75 | 3·300 | ·005 | 45·00 | ·01 | 4·81 | 88·07 |
| 10 | 47·33 | ·12 | 44·33 | 3·267 | ·008 | 44·58 | ·01 | 5·78 | 88·70 |
| 15 | 46·81 | ·16 | 43·84 | 3·229 | ·010 | 44·10 | ·02 | 6·69 | 89·11 |
| 20 | 46·24 | ·20 | 43·30 | 3·188 | ·013 | 43·56 | ·02 | 7·53 | 89·43 |
| 25 | 45·62 | ·24 | 42·72 | 3·144 | ·015 | 42·98 | ·02 | 8·29 | 89·70 |
| 30 | 44·97 | ·27 | 42·11 | 3·098 | ·017 | 42·37 | ·03 | 8·97 | 89·92 |

| Greenwich Noon. | Equatoreal Diameter. | Def. prec. l. | Polar Diameter. | Difference between Limbs in A.R. | Def. prec. l. | in Decl. | Def. south l. | $d$. | $w$. |
|---|---|---|---|---|---|---|---|---|---|
| | ″ | ′ | ″ | s | s | ″ | ″ | ° | ° |
| 1882, Jan. 4 | 44·29 | ·31 | 41·47 | 3·051 | ·019 | 41·73 | ·03 | 9·56 | 90·09 |
| 9 | 43·59 | ·34 | 40·82 | 3·003 | ·021 | 41·07 | ·03 | 10·06 | 90·23 |
| 14 | 42·89 | ·36 | 40·16 | 2·955 | ·023 | 40·41 | ·03 | 10·48 | 90·35 |
| 19 | 42·18 | ·38 | 39·50 | 2·907 | ·024 | 39·75 | ·03 | 10·81 | 90·46 |
| 24 | 41·48 | ·39 | 38·85 | 2·860 | ·025 | 39·09 | ·03 | 11·06 | 90·55 |
| 29 | 40·79 | ·39 | 38·20 | 2·814 | ·025 | 38·44 | ·03 | 11·23 | 90·63 |
| Feb. 3 | 40·12 | ·39 | 37·57 | 2·770 | ·025 | 37·80 | ·03 | 11·32 | 90·69 |
| 8 | 39·47 | ·38 | 36·96 | 2·727 | ·025 | 37·18 | ·03 | 11·33 | 90·75 |
| 13 | 38·84 | ·37 | 36·37 | 2·686 | ·024 | 36·58 | ·03 | 11·27 | 90·80 |
| 18 | 38·23 | ·36 | 35·80 | 2·646 | ·023 | 36·00 | ·03 | 11·14 | 90·85 |
| 23 | 37·65 | ·34 | 35·26 | 2·609 | ·022 | 35·45 | ·03 | 10·95 | 90·89 |
| 28 | 37·10 | ·32 | 34·74 | 2·574 | ·021 | 34·93 | ·02 | 10·70 | 90·93 |
| Mar. 5 | 36·58 | ·30 | 34·25 | 2·541 | ·020 | 34·43 | ·02 | 10·39 | 90·96 |
| 10 | 36·09 | ·28 | 33·79 | 2·511 | ·018 | 33·96 | ·02 | 10·03 | 90·99 |
| 15 | 35·63 | ·25 | 33·36 | 2·482 | ·017 | 33·53 | ·02 | 9·62 | 91·03 |
| 20 | 35·20 | ·23 | 32·96 | 2·456 | ·015 | 33·12 | ·01 | 9·17 | 91·06 |
| 25 | 34·80 | 0·20 | 32·59 | 2·432 | 0·013 | 32·73 | 0·01 | 8·68 | 91·09 |

The inclinations $\gamma$ and the ascending nodes $\Gamma$ of the orbits of the four satellites in reference to the plane of *Jupiter's* equator are the following, the nodes being reckoned from O, the point of the vernal equinox of *Jupiter's* northern hemisphere:—

| 1881 | Sat. I. $\gamma_1$ | Sat. I. $\Gamma_1$ | Sat. II. $\gamma_2$ | Sat. II. $\Gamma_2$ | Sat. III. $\gamma_3$ | Sat. III. $\Gamma_3$ | Sat. IV. $\gamma_4$ | Sat. IV. $\Gamma_4$ |
|---|---|---|---|---|---|---|---|---|
| | ° | ° | ° | ° | ° | ° | ° | ° |
| June 8 | 0·0113 | 351·6 | 0·4736 | 357·53 | 0·1776 | 263·22 | 0·3281 | 330·42 |
| Aug. 7 | ·0113 | 350·4 | ·4744 | 355·60 | ·1763 | 262·81 | ·3276 | 330·56 |
| Oct. 6 | ·0114 | 349·2 | ·4752 | 353·67 | ·1749 | 262·38 | ·3270 | 330·68 |
| Dec. 5 | ·0114 | 348·0 | ·4760 | 351·75 | ·1736 | 261·93 | ·3263 | 330·80 |
| 1882 | | | | | | | | |
| Feb. 3 | ·0113 | 346·7 | ·4766 | 349 82 | ·1723 | 261·47 | ·3256 | 330·90 |
| April 4 | 0·0113 | 345·3 | 0·4772 | 347·89 | 0·1711 | 260·98 | 0·3248 | 330·98 |

If these values of $\Gamma$ are added to the elongations O—L of the point O from superior conjunction, given in the first table, the angles $\Gamma$+O—L are the elongations of the ascending nodes of the orbits from superior conjunction. Hence the latitudes of the satellites above the plane of *Jupiter's* equator are easily found, if their elongations are known.

The following is Prof. Pritchett's communication:—

"Transits of Red Spot across the apparent central Meridian of *Jupiter*. Observations made at Morrison Observatory, between May 25, 1880, and Feb. 22, 1881, by C. M. Pritchett," to which I have added the corresponding longitudes of the middle meridian according to the system of longitudes adopted in the present ephemeris. The times are already reduced Greenwich mean times. The weights are on a scale from 1 to 5.

| | Observed Passages. Prec. End. | Centre. | Foll. End. | Wt. | Corresponding Longitudes. Prec. End. | Centre. | Foll. End. |
|---|---|---|---|---|---|---|---|
| 1880 | h m | h m | h m | | ° | ° | ° |
| May 25 | | 23 12·1 | | 3 | | 356 7 | |
| June 6 | 22 34·6 | 23 5·6 | | 3 | 337·8 | 356·5 | |
| 28 | | | 21 32·3 | 3 | | | 7·9 |
| July 10 | 20 25·7 | 20 59·7 | 21 31·8 | 5 | 332·4 | 352 9 | 12·3 |
| 12 | | 22 40·5 | 23 13·7 | 4 | | 354·7 | 14·7 |
| 19 | 22 48·3 | 23 21·8 | | 3 | 332·3 | 352·6 | |
| 22 | 20 18·9 | 20 52·4 | 21 26·3 | 4 | 333·3 | 353·6 | 14·1 |
| 24 | | 22 29·3 | 23 1·3 | 3 | | 353·1 | 12·4 |
| 29 | 21 5·8 | 21 36·8 | 22 6·5 | 4 | 334·8 | 353·6 | 11·5 |
| 31 | 22 39·8 | 23 13·3 | 23 45·8 | 4 | 332·6 | 352·8 | 12·5 |
| Aug 10 | 20 58·8 | 21 29·8 | 22 1·8 | 4 | 336·2 | 354 9 | 14·3 |
| 12 | 22 34·3 | 23 5·5 | 23 37·6 | 4 | 335·0 | 353·9 | 13·3 |
| 17 | | 22 12·0 | 22 43·1 | 4 | | 354·0 | 12·8 |
| 18 | 17 27·8 | 18 0·1 | 18 30·3 | 5 | 332·7 | 352·8 | 10·5 |

| | Observed Passages. | | | | | Corresponding Longitudes. | | |
|---|---|---|---|---|---|---|---|---|
| | Prec. End. | Centre. | Foll. End. | Wt. | Prec. End. | Centre. | Foll. End. |
| 1880. | h m | h m | h m | | ° | ° | ° |
| 23 | 16 32·2 | 17 3·8 | 17 34·3 | 4 | 331·5 | 350·7 | 9·2 |
| 30 | 17 20·3 | 17 52·8 | 18 23·3 | 4 | 334·4 | 354·0 | 12·5 |
| Sept. 3 | | 21 12·8 | 21 44·7 | 3 | | 357·1 | 16·4 |
| 4 | 16 26·8 | 16 58·8 | 17 29·8 | 5 | 334·7 | 354·1 | 12·8 |
| 5 | 22 14·3 | 22 46·3 | 23 17·8 | 4 | 335·3 | 354·7 | 13·7 |
| 9 | 15 32·8 | 16 4·9 | 16 35·3 | 4 | 334·8 | 354·2 | 12·6 |
| 16 | 16 17·3 | 16 49·3 | 17 19·8 | 5 | 335·6 | 354·9 | 13·3 |
| 23 | 17 4·3 | 17 36·3 | 18 7·3 | 5 | 337·9 | 357·2 | 16·0 |
| 26 | 14 30·1 | 15 2·8 | 15 34·3 | 4 | 336·3 | 356·1 | 15·1 |
| 28 | 16 10·2 | 16 41·5 | 17 12·8 | 4 | 338·0 | 356·9 | 15·8 |
| Oct. 1 | | 14 9·8 | 14 41·4 | 5 | | 356·8 | 16·0 |
| 3 | | 15 52·5 | | 3 | | 0·1 | |
| 5 | 16 54·9 | 17 27·4 | 17 58·3 | 5 | 338·9 | 358·5 | 17·2 |
| 8 | 14 21·6 | 14 53·3 | 15 24·8 | 4 | 337·9 | 357·0 | 16·1 |
| 10 | 15 59·3 | 16 32·1 | 17 2·3 | 4 | 338·0 | 357·9 | 16·1 |
| 13 | 13 30·6 | 14 0·8 | | 3 | 339·8 | 358·2 | |
| 17 | 16 41·8 | | | 3 | 337·5 | | |
| 22 | | 16 20·6 | 16 52·4 | 4 | | 357·2 | 16·5 |
| 29 | 16 33·8 | 17 5·8 | 17 36·9 | 5 | 338·7 | 358·1 | 16·9 |
| Nov. 6 | 13 12·8 | 13 43·7 | | 3 | 341·0 | 359·6 | |
| 13 | 13 52·9 | 14 24·7 | 14 54·1 | 5 | 338·3 | 357·5 | 15·3 |
| 15 | 15 33·6 | 16 3·9 | | 3 | 340·0 | 358·3 | |
| 18 | 13 0·8 | 13 33·0 | 14 3·8 | 3 | 338·9 | 358·3 | 16·9 |
| 20 | 14 36·3 | 15 8·9 | 15 39·3 | 3 | 337·4 | 357·1 | 15·5 |
| 22 | 16 19·0 | 16 50·1 | 17 20·3 | 4 | 340·2 | 359·0 | 17·3 |
| Dec. 2 | 14 32·9 | 15 2·8 | 15 33·3 | 4 | 339·7 | 357·8 | 16·3 |
| 12 | 12 48·7 | 13 19·9 | 13 50·8 | 5 | 340·0 | 358·9 | 17·6 |
| 14 | 14 30·0 | 14 59·5 | 15 31·7 | 5 | 341·9 | 359·7 | 19·2 |
| 1881 | | | | | | | |
| Jan. 5 | 12 38·4 | 13 9·3 | 13 40·6 | 5 | 340·3 | 358·9 | 17·8 |
| 10 | | 12 20·5 | 12 50·8 | 3 | | 0·6 | 18·9 |
| 12 | | 14 0·8 | | 3 | | 1·6 | |
| 15 | 10 57·3 | 11 28·6 | 11 57·8 | 3 | 341·4 | 0·3 | 18·0 |
| 26 | 15 3·1 | 15 34·3 | | 4 | 342·2 | 1·0 | |
| 27 | 10 53·0 | 11 24·3 | 11 55·3 | 5 | 341·2 | 0·2 | 18·9 |
| Feb. 10 | 12 31·3 | 13 0·4 | 13 30·5 | 5 | 343·2 | 0·8 | 19·0 |
| 15 | 11 44·3 | 12 13·3 | 12 41·8 | 4 | 345·7 | 3·2 | 20·4 |
| 22 | | 13 5·3 | 13 33·8 | 3 | | 5·8 | 23·0 |

*Addition to a Paper on the "Effect on the Moon's Movement in Latitude produced by the slow Change of Position of the Plane of the Ecliptic."* By Sir G. B. Airy, K.C.B., Astronomer Royal.

In the paper in the *Monthly Notice* of March 1881 on the "Effect on the Moon's Movement in Latitude &c.," there is a trifling omission.

In page 265, line 3, the value to be used for R is not the radius vector of the centre of gravity of the luno-terrestrial system round the Sun, but $R_m$, the Moon's distance from the Sun. The former has inadvertently been used.

For correction, the expression of the force acting on the Moon must be multiplied by

$$\left(\frac{R}{Rm}\right)^3, \text{ or } 1 + \cdot00780 \cdot \cos\left|\overline{nt - Nt}\right|.$$

This introduces into the final equation the three terms

$$+ \cdot0083930 \cdot Bt \times \{ + \cdot00390 \cdot \sin\left|\overline{2nt - Nt - C}\right| - \cdot00390 \cdot \sin\left|\overline{2nt - 3Nt + C}\right| + \cdot00780 \cdot \sin\left|\overline{Nt - C}\right|\}$$

which are not much increased in integration.

In magnitude, these terms are unimportant. Theoretically, I can hardly conceive such terms to exist. It seems not improbable that they are neutralised, as to their effect normal to the instantaneous ecliptic, by the resolved part of forces parallel to the fixed ecliptic. I have not had leisure to examine into this point.

*Royal Observatory, Greenwich:*
1881, *April* 19.

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*Note on the Physical Libration of the Moon.* By Dr. E. Hartwig.

Die am hiesigen Heliometer ausgeführte Beobachtungsreihe für Bestimmung der physischen Libration des Mondes ist in den *Monthly Notices of the Royal Astronomical Society* mehrfach erwähnt worden, und in dem neuesten Hefte findet sich über dieselbe eine Note von Professor Pritchard. Dieser Umstand veranlasst mich zu einigen nachträglichen Bemerkungen, welche ich der Royal Astronomical Society vorzulegen mir erlaube.

Bei der Ausgleichung der Bedingungsgleichungen nach der Methode der kleinsten Quadrate habe ich in der Berücksichtigung des Gewichtes der Breitengleichungen gegenüber der Längengleichungen einen Fehler begangen. Die Normalgleichungen in Breite habe ich nämlich statt mit dem Gewichte selbst mit dem Quadrat des Gewichtes multiplicirt, welcher Fehler weniger die

Werthe der zu bestimmenden Unbekannten, als vielmehr die Werthe ihrer wahrscheinlichen Fehler, d. h. die Schätzung der Genauigkeit beeinflusst.

Die richtige Ausgleichung mit dem aus der zweiten Ausgleichung sich ergebenden Gewichtsverhältnisse 2, 3 : 1 der Breitengleichungen zu den Längengleichungen liefert die folgenden Werthe der acht Unbekannten, welche von den früheren nicht um $\frac{1}{10}$, im Maximo bei dem Werthe von F $\frac{1}{7}$ des wahrscheinlichen Fehlers abweichen:

| | | | w. F. |
|---|---|---|---|
| F | 1° 36′ 39″ | | ± 139″3 |
| λ | −5 10 58 | | ± 23·8 |
| β | −3 10 23 | | ± 12·5 |
| $a_0 \sin A_0$ | + | 62·3 | ± 22·8 |
| $a_0 \cos A_0$ | − | 0·9 | ± 35·1 |
| $b_0 \sin B'$ | + | 294·6 | ± 131·7 |
| $b_0 \cos B'$ | + | 56·4 | ± 47·6 |
| F | + | 954·8 | + 113·4 |
| $f$ | | 0·507 | |

Der wahrscheinliche Fehler einer Beobachtung ergibt sich zu ±88″·2, von der Erde gesehen ±0″·40.

Der gleiche Fehler ist auch in der früheren Königsberger Reihe begangen worden. Dort sind also die Breitengleichungen statt mit dem Gewichte $\sqrt{10}$ mit dem Gewichte 10 behandelt worden. Die richtige Ausgleichung mit dem Gewichtsverhältnisse 2, 7 : 1 ergibt die folgenden Werthe:

| | | | w. F. |
|---|---|---|---|
| F | 1° 32′ 39″ | | ± 255″5 |
| λ | −5 13 22 | | ± 49·8 |
| β | −3 11 3 | | ± 17·2 |
| $a_0 \sin A_0$ | − | 59·8 | ± 50·6 |
| $a_0 \cos A_0$ | + | 173·2 | ± 63·0 |
| $b_0 \sin B'$ | − | 12·0 | ± 229·0 |
| $b_0 \sin B'$ | + | 16·9 | ± 116·9 |
| F | + | 671·8 | ± 192·2 |
| $f$ | | 0·357 | |

Der wahrscheinliche Fehler einer Beobachtung selenocentrisch folgt zu ±132″·2, geocentrisch + 0″·61.

*Strassburg:*
1881, *Mai* 4.